# TRANSACTIONS

OF

# The Optical Society,

LONDON.

SESSION 1913-1914.

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VOL. XIV.

PART I.

# LECTURE ON VISUAL OPTICS.

BY

MR. JOHN C. EBERHARDT

(Past President of the American Optical Association).

# TRANSACTIONS

OF

# The Optical Society,

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PART II.

# SOME NEW OPTICAL INSTRUMENTS.

BY

BY CHARLES V. DRYSDALE, D.Sc., M.I.E.E.

# TRANSACTIONS

OF

# The Optical Society,

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PART III. TO END.

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# A LECTURE ON VISUAL OPTICS.

It is the source of much gratification to me to have the privilege of appearing before men and women who are engaged in a work which has to do with one of Nature's choicest gifts. We should all realise, as you undoubtedly have in your work, not only the interesting phases that daily present themselves to you in the work which you have chosen, but we all, as we progress, and as we learn to analyse some of the marvels which are demonstrated to us by Nature in this wonderful function of vision, should realise more and more the responsibility that rests upon us in the effort intelligently to discharge the duties we have assumed.

This great responsibility, however, should act simply as a spur to urge us on to higher accomplishment, for, after all, the greatest opportunity and the greatest blessing that can come to any of us is to be so fortunate as to find our field of activity along those lines which are not only agreeable and interesting to us, but which enable us to contribute to the comfort of our fellow man. Another thought which sooner or later always comes to the individual engaged in work of this kind, is that true success is not in the accumulation of wealth, because the millionaire, with all his millions, ultimately is carried out and deposited in mother earth, and eventually forgotten. The real success lies in doing something, however humble, however small, but something definite, which leaves the world just a little richer because of your having lived. Now that is within the reach of all of us. We are engaged in a work which is as yet in its infancy. The first chapters in Optics, as applied to human vision, are really just being written. It is true that if you refer to the chapter on "Light," in the "Encyclopædia Britannica," you find that as early as the beginning of the Christian era—I think it was about 50 A.D.—the great philosopher Ptolemy made his first researches in reflection, refraction, and dispersion of light, and wrote a great volume which, for 1,200 years, was the standard of information to all the world, and of which Professor Tait, of Edinburgh University, has said that for volume of information and accuracy of data, it was unsurpassed in its time. And so we have had many thinkers, and writers, who have written profoundly and knowingly on Optics. But we are entering on a new phase in Optics in which the optician really becomes the pilot who guides light into the harbour within the eye, by means of lenses when necessary, to the end that light emanating from external objects may be placed upon certain portions of the retina in such a manner that when transmitted to the visual centres, a single and well-defined image of such objects shall there be formed. So we are arriving now at a point where we must consider vision more largely from its binocular standpoint; and the possible errors which may be encountered there, and which evidence themselves frequently as factors in disturbances, causing discomfort, are of such profound significance that they are worthy of our most painstaking consideration.

Practically, up to this time, we have dealt with the human eye as a camera, and we have endeavoured, by various means, to ascertain whether or not Nature has equipped a particular individual with a pair of cameras which are as nearly as possible in conformity with normal conditions—which means, as you know, that an eye which, with all its functions in a state of rest, is capable of forming a well-defined image of an external object—provided that object is at least 20 feet away. Splendid work has been done to facilitate our investigations, and to make these researches possible. For many years our efforts were almost exclusively devoted to the subjective investigation. We depended entirely on the subject's answers as to what he did see and how he saw it in making our analysis of his visual condition, all of which would have been extremely satisfactory were the eye a stationary apparatus.

Unfortunately (or fortunately), however, in its construction it is plastic; it is provided with refracting media of a vacillating and constantly changing character, and is frequently very difficult to subjugate into that condition of acquiescence which will enable us, with any degree of accuracy, to say that a given correction, a given combination of lenses, really represents the degree of error from the normal existing in that eye. Much progress has now been made in our objective methods, where we allow the eye to be absolutely passive, and where we work by means of dominating the eye, by forcing it to accomplish some definite task, either by keeping that eye fixed upon certain letters at infinity, or compelling it to read letters at different distances up to fourteen inches.

When an eye reads, for example, letters of a given size, at a distance of one meter, or a third of a meter, we know that a normal eye, under those conditions, would have to develop a given amount of muscular interocular activity in order to change, or to increase, its own refractive power to compensate for the distance at which vision is being accomplished. Professor A. J. Cross, of Columbia University, New York City, one of our leaders in America, who for the past fifteen years has contributed much of his energy to the development of the highest phases of optics, some years ago developed what he termed his dynamic method of measuring the eye with the mirror, as distinguished from the static method of having the eye fixed upon the test chart, twenty feet away. His work has opened an entirely new field, and has laid the foundation for vast progress in the higher phases of our work.

The contributions of Javal and others in measuring corneal curvatures and enabling us to detect and estimate departure from the normal in this part of the optical refracting equipment, you are all no doubt thoroughly familiar with, and these contributions have been admirably amplified by the splendid Sutcliffe instrument for the same purpose. All of these methods have for their end the ability to measure the eye as a camera—to determine whether it is of proper proportion and size, and whether its magnifying apparatus is of the requisite dioptric power. After we have accomplished all this we have not by any means completed the work that is before us.

For the past fifteen years my work has been the study of the co-activity of the two eyes in accomplishing single vision. We know that each eye forms a picture of the object under observation. We also know that if the two eyes were rigidly fixed in the head, so that they could not be turned from their primary position, we would see everything WITHIN INFINITY double. As a consequence, in the study of the binocular function of the human eye, we realise that the baby, in following the ball rolling over the floor, is being given its first lesson by Nature in disciplining the associate muscular functions of the two eyes, to the end that those two eyes, under all conditions, so long as it lies within the resources of the muscular adjustments of the two eyes, will always, involuntarily *and with ease*, assume that position where these two pictures when transmitted to the visual centres will be so perfectly superimposed one over the other that a single well-defined image will result.

If you will consider the philosophy of the stereoscope, where you have a double photograph, and where the prismatic spherical lenses through which you are viewing these deflect the angles of vision so that they are fused into one, producing the splendid perspective effect we all are familiar with, you have an excellent demonstration of the demands made on the human eyes.

Take up a position at any one of your street corners in a busy thoroughfare, and notice the hundreds of pedestrians going back and forwards, and crossing from one side of the street to the other, the various vehicles travelling back and forwards, and behind all this the buildings as a stationary background. Each eye is taking millions of photographs of that moving picture. That in itself is a wonderful thing—that the eye is capable of taking these photographs involuntarily, and of having the general focus so adjusted that all of these moving objects produce a well-defined picture.

But then comes the second thought—that at the same time that this focusing, or accommodating, apparatus is performing this wonderful task, the group of muscles that turn the eyes in or out, up or down, or rotate them upon their anteri-posterior axes, are doing their work with such marvellous speed and precision that at all times, and under all conditions, they are able to cause the two eyes, involuntarily and with ease, to assume that position and angle, both vertical, horizontal and torsional, that these two pictures will really be so perfectly superimposed one over the other that you have, in the visual centres of your brain, a single well-defined picture. Think that your eyes are performing millions of complicated functions of that kind each day! The student cannot consider the complexity of all these various functions without being appalled by the marvellous mechanical problems that are presented therein.

Now comes the next step. The muscles that converge the eye are provided with an extremely generous amount of activity. Usually we find that these functions will develop fully double the efficiency requisite. In other words, knowing that at one-third of a meter with a given pupillary distance, a given amount of convergence is necessary to centre the eyes on the point under observation by the aid of prisms held in front of the eye, we find that the interni can develop not only this primary amount of convergence, but usually fully one hundred per cent. more, leaving a very generous amount of reserve power. On the other hand, the muscles that raise or lower the eyes have an extremely small amount of activity; these muscles rarely having over 2 to 2½ △ D of efficiency. As a consequence, if there should be even a small vertical discrepancy a trying burden would be imposed, leading to possible disturbances.

Therefore, in our investigations, we find that where we are confronted with eyes which, after all refractive defects have been eliminated, still shew symptoms of discomfort, we must look for some other cause. We must learn to be resourceful, and never stop as long as there is a single avenue open, along optical lines, for us to pursue in order to determine the cause for discomfort.

Consider the eye from the standpoint of a double camera, investigate and ascertain whether or not these two eyes are able simultaneously, involuntarily, and with ease, to convert themselves into a stereoscope.

We are all familiar with the test for horizontal and vertical poise by means of the Maddox rod and a light 20 feet away. It is quite simple, and anyone can, with a little application, familiarise himself with it. Many writers have questioned the value of this test, because they have shewn that in many instances it was absolutely unreliable, and that a case which, under that test, would shew that one or the other eye was below the horizontal, requiring a given amount of prism power to restore horizontalism, has positively refused to accept such prism, its use increasing discomfort.

There was a reason for this. There were those who found a prism necessary for every case encountered, but that prism frequently was like the Irishman's flea: you never knew when you had him! In reading the prescriptions furnished from time to time, it was found that in one prescription the prism appeared base up, the next time base down, the next base in, then base out, and finally it disappeared. Such things did not contribute towards developing confidence in the value of prisms. But there was a reason for it.

I shall present to you a few cases not at all uncommon, and to be encountered by any practitioner who will be on the alert. If intelligently investigated you will find in these the solution of many perplexing cases which come back to you, and which finally baffle your patience as well as your best endeavour. A number of years ago I had my first interesting case, which opened my eyes to this possibility. This was a lady over 60, wearing a concave lens of about minus 6 D, who consulted me with reference to a pair of reading glasses. She complained of constant double vision, which proved very distressing. On inquiry, I found that as a result of a very severe fall which she had had some years before, this double vision had developed, that various forms of treatment had been tried, and that ultimately an operation had been suggested, to which the family did not agree, and which the family physician disadvised owing to her advanced age. On investigation I found that the left eye drooped 8 △ D. In other words, it required an 8 △ D prism, base up, in order to bring the two retinal images in line. You can understand that I hesitated somewhat to prescribe anything so radical in conjunction with a minus 6 D lens—which was combined with a cylinder, by the way. As a consequence, I made a number of repeated examinations, and finally prescribed a pair of lenses as an extra pair, purely for experimental purposes. She fully understood that it was for the purpose of determining whether or not the conditions could be improved. In these I embodied one-half the amount of prism power I have mentioned, that is, I added a 4 D prism in her correction. She accepted it with much comfort. She still had some double vision, but she said that part of the time, at least, she was able to walk around without discomfort, although objects still doubled somewhat.

I kept her under observation, and gradually increased the prism, finding that comfort resulted, until finally she was wearing a 6 D prism, which I divided, giving her one-half base down for one eye and one-half base up for the other. With that amount of correction she was perfectly happy.

One day she came to me and said, "My distance glasses are satisfactory, but I cannot use my reading glasses owing to the troublesome blurring of the print." I carefully verified my previous examinations, and found that the condition of her eyes was just as before, although the six degrees of correction gave her single vision, Nature, no doubt, being able to supply the other two degrees.

I found that the refractive power of reading lenses was not to be improved upon, but she could not obtain comfortable binocular vision with these combined with the prism correction I had provided. Suddenly the thought suggested itself to me: "Is it possible that the vertical discrepancy can be modified when she assumes the position that she does in reading? In other words, is it possible that although her eyes, when looking straight forward, shew this amount of vertical discrepancy, the amount of that discrepancy can be different under reading conditions?" I proceeded to investigate, making various examinations at different times.

Finally, I devised a card, copies of which I shall be pleased to give to the members of this Society. On this card I had printed a line of test letters about the size of the small reading letters on your reading test card—just a single line of type which says, "Are the three lines parallel, equidistant, and what are the relative positions of the three dots?" In front of that there was printed a dot about ⅛in. in diameter. I placed the reading correction for this lady, when she came in on the following day, in the trial frame, and the opaque disc before the left eye, placing over the right eye the double Maddox prism in a horizontal position. The card, containing the single row of letters, was placed on the reading chart, and held in a natural position at her normal reading distance; then, by gently raising or lowering the head, she assumed that position where the eye intersected the centre of the double prism.

When she accomplished this, double vision was produced, and as a consequence she now saw two lines instead of one, one being deflected up by one prism, and down by the other. Holding her head in this position, I gently drew the opaque disc from the left cell when she should have seen the third line equidistant between these two lines.

I had inserted before the left eye at random the 6 △ D prism being used in her distance correction, when with some difficulty she located the third line, but said "It is almost up against the top line." I had to decrease the prism until finally, with a 3½△ D prism, base up, before that eye, she found that lines were equidistant.

In other words, for some mysterious reason, her eyes which, when looking straight forward, developed a vertical imbalance of 8 △ D, when adjusted for reading only indicated 3½△ D. I have no theory to advance, because I have found exactly the opposite conditions in some cases.

This occurred eight years ago, and during this time I have frequently re-examined her. The same discrepancy exists to-day. She only wears a 3 △ D correction in her reading lenses, and reads with perfect comfort, whereas for distance she still requires 6 △ D. The method of investigation is so simple that where you have a case in which you have exhausted every other avenue, and yet there is still discomfort, it is certainly worth while to ascertain whether or not there is harmonious co-activity between the two eyes when these are forced to work together either at reading distance or at infinity.

I have had numerous instances which had been in the hands of very painstaking operators, who had investigated the muscular aspect, and had found nothing wrong, yet marked discomfort was present. I have now in my mind one case of a book-keeper who continually had headaches. He had had all sorts of refractive corrections, and complicated astigmatic conditions had been discovered by some examiners. On a subsequent occasion, after we had solved the difficulty, I saw a number of his prescriptions, which shewed that astigmatic conditions are sometimes just about as erratic as the Irishman's pest, and are not to be depended upon always. And yet that man had marked discomfort. The most painstaking examination that I made failed to reveal to me any refractive error of sufficient amount to be responsible for his discomfort.

We do not find any perfect eyes—there is not an eye that under critical examination will not develop some departure from the normal. But Nature compensates for the small errors, as a rule. Sometimes she does not, but where there are ample resources in the various departments, and there is a small error present, Nature usually takes care of that error.

In his case a careful examination failed to reveal any refractive error. He had ample accommodation, and he had the same amount of accommodation in each eye. He had ample convergence, and ample reserve power under all tests. With the Maddox rod at 20′, there was a perfect condition of equipoise, horizontal and vertical, and so far as any cyclophoric disturbance was concerned; and yet, under the test described, made at the reading distance, one eye drooped 2½ △ D below its mate.

I made numerous examinations at different times, and that condition continued. Experimentally I gave him mechanical relief, giving him a crutch for that eye, so as to restore horizontalism. All of his troubles were wiped out. Now we may argue, and we may theorise about the inadvisability of using prisms. A prism is not different from either a concave or a convex lens, because they are all pieces of glass, and serve the purpose of changing the conditions under which light enters the eye, and if its use contributes towards introducing light into the eye under those conditions where it can perform its work with comfort, we are doing good service.

So that you have those two conditions: you have frequently a very marked muscular imbalance at infinity which will be very much modified when examined at the reading angle, while on the contrary there are many cases which at infinity shew absolutely no imbalance, and which do shew a very decided imbalance when investigated at 14″ at the working angle. And where you find any departure from that mechanical adjustment which should prevail, correct it experimentally, and if it gives comfort, and the eye accepts it readily, and it wipes out the difficulties which have existed—give it. That seems good logic.

There is another very interesting phase which I have very frequently encountered, and which has been a source, in many instances, of a great deal of discomfort, and I will again cite a definite case. About seven years ago the daughter of the President of one of our Universities was brought to me with a history of headaches which had developed during the past two years, and which had gradually grown worse. She had been refracted by some of our most capable men, and was wearing a slight refractive correction, which, however, did not give relief. Her headaches were of an accommodative character; that is, during her vacation she had no headaches, they were only produced by the continuous close use of the eye. This led the physician to determine that the eye must be the causative factor.

My most critical examination failed, in her case also, to reveal any refractive error that could possibly be causing this acute disturbance. The eye itself presented a perfect condition of health. A very critical examination of the fundus failed to reveal any disturbance there. There was ample accommodation, and equal accommodation in both eyes. All the muscles seemed to be in a normal condition of activity. A critical investigation of the equipoise shewed the eyes to be in a perfect condition of equipoise in all directions at infinity. When, however, the test at reading distance was made, I discovered a very interesting phenomenon. It has been written on extensively since, in a recent work of Dr. George A. Stevens, of New York City, on "The Motor Apparatus of the Eye"—in my judgment the most magnificent contribution to that phase of the science of vision that has been given to the world at this time—and he writes an exhaustive chapter, and very interestingly explains the phenomenon that I am going to present to you now. As I say, up to this point, this young lady, who was about 18 years of age, and was apparently in good health with the exception of constant headaches when eyes were used at reading distance, had exhibited absolutely no visual error of any kind.

When I made this examination, and placed the double Maddox prism before the right eye until she saw these two lines, and then uncovered the left eye in order to see what position the third line would occupy, it was found that the two eyes not only crossed to a marked degree but shewed a decided obliquity.

I found, of course, this demonstrated not only that there was a very marked obliquity, but there was a very marked over-convergence, there was a crossing of the visual axes, and the actual measurements shewed me that in order to bring the convergence up to where it ought to be, there was an over-stimulation of 20 △ D. As a consequence, whenever the eyes were used at the reading distance she was forcing the externi, which, as you know, have only a limited amount of efficiency, by the aid of the other related abductors, to counteract this excessive convergence. It was not difficult to understand why she was suffering, but what was the remedy? There was no refractive error to correct; you could not put a prism there. What could be done? An over-stimulation of the converging function was, of course, the cause. The thought occurred to me that if by some means the innervation could be inhibited, possibly this excessive convergence could be neutralised.

Knowing the intimate associate activity existing between convergence and accommodation, I at random slipped a pair of plus 1½ D spheres before the eyes. At first she was forced to bring her print rather close. Gradually relaxation took place, enabling her to read at about 13 in. After a moment she stated that the lines were straightening out, assuming a more nearly parallel position, she ultimately accepted a pair of +2.00 D spheres O.U. FOR READING. By the aid of these two diopters of accommodation were inhibited, she reading print at 14 in. with only 1 D of accommodation.

I now will tell you the sequel. She had been ordered by her physician to abandon all idea of entering the University, for which she was preparing at that time, and, for at least one year, to stop all close use of the eyes. She went back and completed her examinations. I saw her once a year after that; she not only went through her University with high honours, but during the last two years did special work during the summer months. She has maintained good health, and has had no headaches.

Now, gentlemen, here we employed lenses not for the purpose of enabling her to see, but to discipline the nerves which were involved in the disturbance; and it presents an interesting possibility, because by the aid of a piece of glass you can dominate the several ocular functions, either by stimulating or inhibiting them.

The following case presents diametrically opposite conditions. A gentleman, aged 51, presented himself for examination. He stood 6 ft. 2 in., was a lumber buyer, spent his life in the open, for several years had suffered from headaches, which were induced by close use of the eyes. Refracted him with the greatest care, and discovered 2 D of hyperopia, and that he required plus 2¼ D added (or a total of 4¼ D) reading. I found a perfect condition of muscular poise. Critical examination of the fundus failed to reveal any disturbance, and much to my chagrin, when I looked at the glasses he was wearing, they were a pair of plus 2 above and a pair of 4¼ below.

You can imagine I did not feel very comfortable, because in this case I was very anxious to please the physician who had sent him to me. There was a condition of horizontal and vertical poise, and the test both for reading and at a distance of twenty feet proved satisfactory. The final investigation, much to my surprise, revealed the error. A vertical row of letters, in which these are printed one below the other—"Minimum and maximum convergence." With his reading correction on, I had him hold this card at 14 in., and while slowly reading them I took a 5 D prism, and slipped it base out in front of the right eye over his reading correction. "Why," he said, "there are two lines." I said, "Draw them together." But by the greatest effort he was unable to fuse the two lines. After repeated efforts it was found that he had less than 3 △ of reserve convergence; with a 2 △ D prism, by a supreme effort, he with difficulty fused the two lines.

After repeated examinations I found that for some mysterious reason the converging power was absolutely insufficient, and that when reading he was forcing the converging muscles to develop their maximum tension. I made two subsequent examinations before arriving at my decision.

One of the great mistakes, in my opinion, made by many refractionists, is that they are entirely too anxious to finish up a case at the first sitting; this is bad. If necessary, give a preliminary pair of lenses, but keep the case under observation, making a number of examinations, keeping detailed records of each, before the final correction is supplied. We should develope a tendency towards more deliberation. After I had seen this case four different times, and finding no change, I ordered a pair of bifocal glasses, precisely the same as those he was wearing, 2 D above, with a plus 2¼ D added below, but embodied a pair of 3 △ D prisms, base in, O.U. In other words, I gave him a pair of crutches, with the instructions that he was to wear his old lenses for outdoor use, and to wear the pair I was prescribing for office work only.

A few weeks later he reported that for the first time in four years he had been able to do his office work, and read evenings without developing headaches, and this condition has continued.

The measurement of convergence efficiency should always be a part of the regular routine. You cannot make your record of a case too complete. The more data you have the more you have got to build your judgment upon.

Now, I want to present to you the other thing which I have found not at all uncommon, and possibly the description of a single case will serve best, though I have many of them. A young girl of 15 was sent in to me with a history of headaches during school. She had no headaches at any other time. She was wearing a pair of $\frac{1}{4}$ cylinders, slightly oblique, which had no value. She had been examined repeatedly under drug by competent men. Slight changes had been made in her prescriptions. The headaches went right off. She was advised to leave school.

The most careful refraction failed to reveal any refractive error, though possibly there was a slight one, because, as I say, I doubt whether you can bring me an eye in which I cannot shew you an error. You do not find perfection in any part of the human anatomy; there are imperfections present, but there are always functions to compensate for slight imperfections, because Nature anticipates imperfections. But to all intents and purposes, in the case of this young girl, a perfect condition, as nearly as possible, of visual faculties prevailed. Muscular conditions were normal, both at infinity and at reading distance. Convergence was normal. All the muscles were in a normal condition. Naturally, I made my other examination, which, no matter whether it is a child or whether it is an old man, is always a part of my routine. I do not care if he is a presbyope; I want to know, and our work is not scientific unless we do know, how much accommodation, if any, has each eye got, or what loss of accommodation exists in each eye, and is it equal. We have been in the habit, in recent years, of devoting a great deal of time to the investigation of refractive errors, and after we have got them, if the person happens to be a presbyope, we at random make a superficial test, and add so MUCH O.U. This is all wrong; it is absolutely unscientific, and the study of the eye at the near point is a thing we have to give more attention to.

In this case I placed the opaque disc before the left eye, and a −2.00 D sph. before the right. Through this she gradually read the fine reading test type. I slipped in a minus 3, and she read it. I slipped in a minus 4; she read it. I slipped in a minus 5, and by a little effort she read it. With a minus 6, she had to make quite an effort, and said she could read it, but the print blurred somewhat. Now what did that mean? Through a minus 6 she was reading at 14 inches. The normal eye when reading at 14 in., we know, requires 3 D of accommodation. When she was reading this print at 14 in., we know that she was developing 3 D of accommodation to read. But I placed a minus 6 over her eye, and as a consequence we know she was developing 9 D of accommodation for that eye.

That is positive evidence that it was there. I knew that to be ample, so I placed the opaque disc in front of her right eye, and had her read the print with the left. I slipped in a minus 2 D before the left eye, and found she could not read the print, through a −1.25 D sphere by a supreme effort she slowly read the print. In other words, the right eye had 9 D and the left eye had the 3 D, which she needed, and the one minus 1½ which I put over her eye, which is a 4½ D, so that there was a discrepancy of 5 D of accommodation between the two eyes.

I investigated, and found that all her trouble dated from a serious attack of scarlet fever, that upon recovery from her fever she began to develop these headaches in going to school, and that as a consequence she was excused from school for another month. Shortly after that, school ended, and the doctor said that by fall it would be all right. That fall the same condition repeated itself, and all that winter she had been suffering with these headaches, and had had four or five examinations, and had several different refractive corrections. I gave the girl, for the right eye, a plus .25 sph., and for the left eye a plus 2½ D sphere. I gave her that which I felt Nature would be grateful for. I gave her a little crutch, and the trouble ceased. I had her under observation from time to time, and made some little modifications. That was about two and a half years ago, and for the last two years she has been wearing a plus 2. Anything less has not been quite comfortable. She has not regained the accommodative faculty in that left eye. Now, prior to that time she had perfect comfort, and her discomfort dated from her disease. It is rational to suppose that the effect of that disease was to produce a degeneration in the crystalline structure which led to its premature degeneration. In other words, she has premature presbyopia caused by disease. What are you going to do about it? Pass it over, or give her assistance? She can use her eyes, and she is comfortable.

Those are just a few points, but upon these philosophies are based endless possibilities, because no two cases are alike. If you will begin, as a matter of routine, to investigate whether two eyes at infinity are in a condition of equipoise, and whether those same eyes assume a position of equipoise at 14 in., and whether there is ample convergence, and whether each eye has an equal amount of accommodation, and whether the accommodation is about what it ought to be—if you will add those things to your regular routine, as a part of your duty, and, where you have discrepancies, correct or assist them, you will find grateful results.

In other words, when you introduce into your work the law of supply and demand, and supply lenses to the end that all functions involved in the accomplishment of binocular vision may be able to perform their work under equally favourable conditions, and under conditions of greatest comfort and of greatest economy, you will be adding a very valuable department to your work, and I know it will greatly add to your results, and to the pleasure of your work.

# TRANSACTIONS
OF
# The Optical Society, 1913-4.
## PART II.

# SOME NEW OPTICAL INSTRUMENTS.

*Read November 13, 1913.*

In bringing the present paper before you, I feel that an apology is due to the Society for my having been obliged to postpone it from last Session, and for my being unable, even now, to show you all I had intended to. Several of these instruments were to have been constructed in the summer of this year, when I was away in Hungary, but on my return I found they were not available, and Mr. Tinsley has very kindly thrown himself into the breach, and has constructed them within the last two months. Most of them are, however, in fairly satisfactory working order, and they will enable you to observe their behaviour.

It should also be mentioned that the title of the paper, "Some New Optical Instruments," is so far a misnomer in that the principle of most of them has been described before. But they have not hitherto been put into working shape, and I think you will find them of sufficient interest to justify their having been brought before you. Perhaps it will be well if I commence with one which has not previously been described, except in a small MS. communication. The subject of colour vision testing has been much before us recently, and no doubt this instrument will afford material for considerable discussion.

## 1. Apparatus for Testing Colour Vision and Colour Blindness.

Numerous as have been the tests and devices for investigating colour vision and colour blindness, there yet seems to be a need for a device which shall be both simple of interpretation and definite in its indications; and the instrument about to be described, which the writer proposes to call a Chromoptometer, has been designed with the object of meeting these requirements. It does not depend on any preconceived hypothesis as to the mechanism or nature of colour vision, but it permits any such hypothesis to be investigated.

In testing the colour perception of any individual we must distinguish between three distinct though connected faculties :—

(*a*) Power of identification and nomenclature.

(*b*) Retinal sensitiveness to light of various colours.

(*c*) Power of discrimination between nearly similar colours.

These three quantities may be most readily appreciated with reference to the analagous sense of hearing. In this case we have (*a*) the knowledge of musical nomenclature, (*b*) the sensitiveness of hearing to sounds of different pitch, (*c*) the power of discriminating between sounds of nearly similar pitch.

Defects in colour sense may be due to deficiency in any one of the above faculties. A child or grown-up person may have eyes which are perfectly normal as regards colour vision, but if he has been in the company of a colour-blind person, and has heard colours systematically wrongly named, he will very probably make the same errors, and thus appear to be colour-blind. On the other hand, the eye may be more or less completely blind—*i.e.*, the retina more or less subnormal in sensitiveness to lights of certain wave length. Thirdly, the eye may be incapable of discriminating between small changes of colour, a defect corresponding to "tone-deafness" in the acoustic analogy.

The chromoptometer has been designed with the object of permitting these features to be tested and recorded in normal and abnormal eyes, so that the departure from the normal in the case of colour-blind eyes may be determined and measured. Like many other instruments for testing colour vision, it is fundamentally a spectroscope, but it is provided with certain accessories in order to enable metrical results to be obtained.

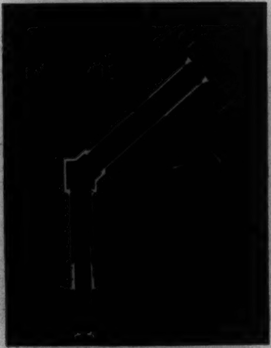

FIG. 1. FIG. 2.

The spectroscope (fig. 1) consists of the usual collimator C, prism P, and observing tube O, but the objective L only half fills the tube, the remaining half having a ground glass screen G. The slits are three in number, one $S_1$ being of variable aperture, and actuated by the screw and milled head, $H_1$, while the other two, $S_2$

and $S_2'$, are of fixed width, but can be moved bodily by the screws and milled heads, $H_2$ and $H_2'$. These are so arranged that when the pointers, $T_2$ and $T_2'$, are together, the slits $S_2$ and $S_2'$ are in line, and they can be traversed bodily by turning the milled head $H_2$; but on turning the milled head $H_2'$, the slit $S_2'$ is moved past $S_2$. The light from the two slits $S_2$ and $S_2'$ passes through the prism P, and forms a spectrum at the slit in the eye-piece, so that on looking through this slit a monochromatic patch of colour is seen at the left hand side of the field. This colour can be changed from red through to violet by turning the milled head $H_2$, and the colour of the upper half can be changed relatively to that of the lower half by turning the milled head $H_2'$. The light from slit $S_1$ falls on the ground glass G, after which it is reflected at the second face of the prism, so that a patch of white light is seen on the right hand side of the field, separated from the coloured patch by the sharp front edge of the prism, and the brightness of this white patch can be varied at will by turning the milled head $H_1$. The pointers, $T_2$ and $T_2'$, move over a scale divided in wave lengths, and having the corresponding colours engraved upon it, while the pointer $T_1$ moves over a scale indicating the width of the slit. A cylindrical lens Cy., with axis horizontal, is inserted into one half of the collimating tube, and serves to secure a sharp line of junction between the upper and lower spectra.

The modus operandi of the apparatus is as follows :—

### A. *Identification and Nomenclature Test.*

The slit $S_1$ is closed, and the slits $S_2$ and $S_2'$ brought together into line (the pointers $T_2$ and $T_2'$ being together). By turning the milled head $H_2$, the wave length of the colour patch is altered, and the subject is asked to name the colours, or he is asked to turn the head until a certain colour appears. This can first be done with a large number of normal sighted persons, and the limits of wave length corresponding to various colours arrived at for average normal vision. These colours can then be marked on the dial, and any departure from them in the case of a new subject indicates the defect in his colour nomenclature.

*B. Retinal Sensitiveness Test.*

For this purpose an attempt is made to measure the apparent luminosity of the colour patch at various wave lengths. To facilitate this a glass discrimination diagram with transparent graduated letters on a black ground is slipped in front of the prism, and the subject is asked to alter the illumination of the white patch by turning the screw, until he can read equally well on either side. The reading of the scale $S_1$ shows the apparent luminosity of the colour patch, and this operation is repeated with various wave lengths, and the spectrum illumination curve, fig. 2, plotted. This is done with a large number of normally sighted individuals, and the average curve obtained. In the case of a defective subject, his curve will show a marked deficiency from the curve at certain wave lengths. If it occurs at the red or green, etc., he is more or less completely "red blind" or "green blind," and there can be no doubt as to the interpretation of the result.

*C. Discrimination or Matching Test.*

For this purpose all that is necessary is to turn the screw $S_2$ until any required colour is obtained, and then to turn the head $H_2'$ in either direction until the subject says that the colour has changed relatively to the bottom one. This gives the limit, $\lambda_2 - \lambda_1 = \triangle\lambda$, over which the colour can be changed before the change is detected. By this means the power of matching colours can be numerically expressed.

It may be objected to the latter test that a colour-blind person would be able to detect the change of wave length, owing to the change of illumination attending it. With normal vision, however, the change of tint can be detected much more readily than the change of intensity, so that in colour blindness it appears as if $\triangle\lambda$ must be greatly increased. The difficulty can, however, be surmounted by putting a graded strip of glass or celluloid in front of the slits.

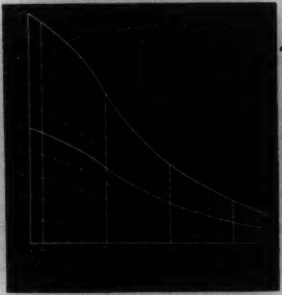

FIG. 3.

As regards the construction of the wave length scale, this is, of course, easily arranged by calibrating with known spectrum lines. Of course, with a prism the scale is not even, and this affects the form of the spectrum luminosity curve. If the apparatus is always made with prisms of a definite glass, the curves will all be comparable. Should it be desirable for any reason to correct the curve so as to correspond with a uniform or grating spectrum, this may readily be done by multiplying each ordinate of the diagram by a factor, $f=a\frac{\delta x}{\delta \lambda}$, where $x$ is the scale reading, and $\lambda$ the wave length. This correction may be automatically affected by plotting the luminosity readings, not on ordinary squared paper, but on a diagram, as in fig. 3, annexed, where the vertical scale at each ordinate is proportional to $\frac{\delta x}{\delta \lambda}$

It is, of course, important that the whole of the apertures should be equally and brightly illuminated with light of a definite colour, but a high candle power tungsten lamp with thoroughly frosted globe appears to serve the purpose very well. The test applies principally, of course, to macular vision, but it should not be difficult to extend it to peripheral vision as well. Preliminary trials of the instrument appear to indicate that it realises the expectations formed of it very well, and the author hopes that it may be found a satisfactory and definite solution of the problem of colour vision testing.

## 2. Universal Optical Bench.

At a meeting of the Optical Society in 1901, the writer described and showed in operation an optical bench designed to deal with all types of spectacle lenses, as well as lens combinations, with rapidity and sufficient accuracy for technical purposes. Although quite satisfactory in its operation, this apparatus was somewhat unwieldy for ordinary commercial use, as it was over seven feet long, and the writer has now modified it so as to halve the length and increase its sensitiveness. This has been done by adopting the auto-collimation principle and reflecting the beam back from a plane or concave mirror at the end of the bench.

In fig. 4 is shown the general arrangement of the bench, from which it is seen that it consists of an open-fronted box, at one end of which a collimator C is fixed. This collimator contains, however, a double right-angled prism P, the hypotenuse of which is lightly silvered and balsamed so as to transmit and reflect light nearly equally. The light from the glow lamp S is concentrated at a small aperture A at one face of the prism, and after passing through the prism and through the collimator, is rendered parallel by the objective O. It then traverses the length of the box and falls upon the plane mirror M, after which it returns as it came, and being reflected at the partially silvered surface, is brought to a focus at the face G of the prism, against which a ground glass screen is placed, observed through a magnifying lens *l*.

FIG. 4.

Along the floor of the box are laid two rails of V and rectangular form respectively, along which traverses a support S, on which the lens to be tested is fixed. Vertical and horizontal adjustments are provided, together with scales which show the amount of vertical or horizontal decentration. A long scale divided in millimeters and diopters runs alongside the rails.

If an ordinary convex spherical lens is mounted on the support, and is traversed along the bench until it focuses the parallel beam on the mirror M, the light will be reflected and rendered parallel after passing back through the lens, and will give a sharp image of the aperture A on the screen, while the focal length and power in diopters can be immediately read off on the scale. The axis of the mirror is marked by a small white spot, and if the lens be adjusted until the focus falls on this spot, its optical centre is the axis of the beam, and it can be marked by a long flexible upright mounted on the base. In the case of cylindrical lenses, a focal line, perpendicular to the axis, is, of course, obtained, instead of a spot of light when out of focus, and as the screen G is provided with a protractor scale, the angle of the axis can be immediately read off. A sphero cylinder with both meridians convergent, of course, gives the two focal lines. Should the power of the lens or of one meridian be less than one diopter, an auxiliary previously tested +1 D lens is placed in a clip practically in contact with the lens under test.

For divergent lenses the procedure is exactly the same, except that a second objective L, which is hinged to the collimator, is brought into action. This lens can be rapidly swung into position and hasped there, and the mirror M is adjusted at the outset so that the focus of the lens falls on the centre of the mirror. The result is that the light is returned and is focused on the screen, equally, whether the lens L is in front of the collimator or not. When it is in position, however, and a divergent lens is interposed between it and the mirror, the light will again be rendered parallel, and a sharp image of the aperture A will be formed on the screen, when the focus of the lens under test is on the mirror. The focal length and power in diopters are then read off on the same scale as in the case of the convex lens. A concave mirror may be substituted for a plane one, and the auxiliary objective dispensed with, but in that case a second scale would have to be employed for convex lenses.* The auxiliary objective could also be dispensed with if the collimator were extended to focus the light on the mirror M by the objective, but the swinging in of the second objective is more convenient, and is optically preferable.†

For the majority of purposes one or other of two lens supports is sufficient. One of these is a straight stem fitting into the stand, and bearing at the top three small spectacle lens clips of ordinary gauge, and a projecting bar. This enables an ordinary pair of spectacles or eyeglasses to be mounted on the bench, and to be kept horizontal while being tested. If an auxiliary lens is required, it can be mounted in one of the other clips. For lenses of other diameter or square lenses, or thick lens systems, the second head can be substituted. This has two wide V-shaped cork-lined jaws, actuated by a right and left-handed screw, so as to be self-centering. This head is arranged to swivel, and is provided with a protractor scale to indicate the angle. By oscillating the head and pushing the lens system through the clips, a stationary image can be obtained, with the nodal point of emergence over the axis of rotation, and the equivalent focal length is then read off on the bench scale, as in the case of a thin lens. The focal length at various angles can also be obtained by the aid of the protractor scale.

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*In the apparatus as shown at the meeting, and as now made, the concave mirror has been definitely adopted as giving a large aperture, and enabling deep concave lenses to be satisfactorily tested. It has been found possible, also, to discard the partially reflecting prisms entirely, so that the bench simply consists of a screen with aperture, a collimating objective, and a concave mirror.

† In the final form of the apparatus a concave mirror 6″ diameter and 1 meter radius has been substituted for the plane mirror M, and the objective L has been dispensed with. It has also been found possible to do away with the reflecting prisms and return the light directly to a screen surrounding the aperture A, so that the optical arrangements of the bench are extremely simple. The focal lines in the case of cylindrical and sphero-cylindrical lenses are then in the direction of the axes.

The radius of curvature of convex polished spherical or cylindrical surfaces can, of course, be instantly obtained by placing them in the convergent beam. By the aid of a concave lens the curvature of concave surfaces can similarly be determined.

To avoid stray light, a curtain is hung from a rod at the top of the front of the box, while to facilitate the movement of the lens under test, the stand is actuated by a cord and pulleys moved by a handle close to the collimator.

### 3. Optical Spherometer.

This apparatus has been designed on the principle of the method of measuring the curvature described before the Optical Society in 1901. Professor Féry, however, informs me that a similar principle has been employed in the laboratory of the École Municipale de Physique et de Chimie Industrielle at Paris, and that it was due to Laurent. I am not aware that any practical instrument has hitherto been constructed on these lines. The method and the present instrument is specially suitable for the testing of the radius of curvature of small polished spherical or cylindrical surfaces, where no mechanical spherometer could be employed, and it has the advantage of measuring the radius of curvature directly without calculation or correction.

The principle of the method is extremely simple, and need only be very briefly recalled. If a convergent pencil of light strikes a spherical reflecting surface, it will be reflected, and if the centre of survature of the surface lies on the axis of the pencil, the latter will be reflected back apparently unaltered (*a*) if its focus is at the surface, and (*b*) if it is at the centre of curvature of the surface tested. If an auto-collimating device be employed, therefore, a sharp image can be obtained in two positions of the surface tested, and the distance moved between these two positions is the radius of curvature required.

The present instrument has been designed to directly and conveniently measure the curvature of surfaces with radii of curvature up to 60 or 70 mms., but the range can readily be extended. It consists of a stand with a vertical bar (fig. 5), on which are fixed an ordinary microscope tube M, and a rising and falling table T, both of the latter being actuated by rack and pinion. The microscope is provided with a Ramsden's eyepiece, and immediately below it is a double prism P with partially cemented face, so as to be both reflecting and transparent.

FIG. 5.

Light falls on this reflecting surface from a pinhole aperture illuminated by a glow lamp fixed in a side tube, and having been reflected down the tube, falls on an ordinary microscope objective. On passing through this the light is rendered convergent, and on being reflected, passes back up the tube, through the prisms and to the eyepiece. To make the reading as easy and direct as possible, a scale is attached to the upright, and a vernier to the microscope body. A click is also provided which holds the latter in the zero position when required.

The surface to be tested, either mounted or unmounted, is placed on the table, and is approximately centered by the aid of concentric rings engraved on the table. The microscope is then racked until the click slips into position, and the table is then racked up and down until a sharp image is obtained with the focus at the surface under test. The click is then released, and the microscope racked until the second image is in focus (upwards for concave, downwards for convex surfaces), and the reading on the scale and vernier is the radius required. Two scales and verniers are provided, reading in millimeters and in inches respectively, and subdivided to 1/10 mm. or 1/200 inch. In the case of cylindrical surfaces, the procedure is identical, but the second image appears as a streak parallel to the axis of the cylinder, while in the case of toroidal surfaces, two such radii of curvature are read off. In the case of transparent surfaces, such as lenses, it is convenient to put a spot of ink on the back, in order to avoid confusion with reflections from the back surface. On the other hand, if a plane mirror is placed below the lens, a bright image is obtained when the point of the optical probe is at the focal point of the lens, and the optical properties of the lens may be very fully investigated in this way.

## 4. Apparatus for Measuring the Magnification Field of View and Illumination of Field Glasses.

Apart from its defining power, the three important quantities which it is desirable to determine in the case of an opera or field glass are its magnification and the extent and brightness of its field of view. The various types of glasses vary so much in these respects that it is eminently desirable for the optician and the public to have some simple direct and objective method of determining them. The apparatus now to be described has been designed for this purpose, and it combines the principles laid down in the author's paper on small telescopes and binoculars read before the Optical Convention of 1905. It consists of a baseboard (fig. 6) upon which are mounted a collimator C, a table for supporting the instrument to be tested, and an observing camera or "artificial eye," C′. The two tubes C and C′ are similar in that they each have ground glass screens fixed at the focal plane of a lens of 5¾ inches focal length, so that divisions of 1/10 inch engraved on the glass subtend an angle of 1° at the nodal point of the lens. When

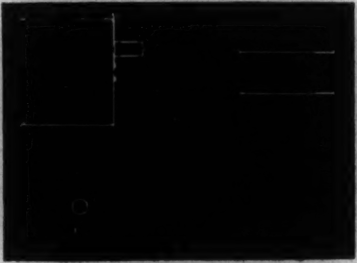

Fig. 6.

the ground glass of the collimating tube is brightly illuminated by a lamp L, the divisions upon it are focused without alteration of size on the screen or "retina" of the camera C′, and correspond in size with those already engraved upon it. If now the telescope to be tested be focused for distant objects in the ordinary way. the parallel rays emerging from the collimator will emerge parallel from the eyepiece of the telescope, and will be focused on the camera screen or "retina" as before, but the angular distance between the division will be increased, and the number of engraved divisions on the the "retina" corresponding to one division of the image gives the magnification directly.

As regards the field of view, the divisions on the "retina" are graduated so as to represent degrees up to 30° on each side of the axis (it would perhaps be preferable for the ground glass to be spherical in form, and the divisions would then be all equal).

When the field glass is interposed, a circular illuminated disc is seen, which represents the field as seen by the eye, the number of magnified divisions visible across the diameter of the disc is the actual field of view of the glass in degrees, while the number of small divisions gives the apparent angle of view as seen by the observer.

It must not be forgotten that the angle of view of a glass of the Galilean type, in which the exit pupil is formed by the pupil of the eye, depends upon the diameter of the latter. The maximum normal diameter of the pupil of the eye may be taken as 5 mm., and the aperture of the lens of the observing camera C′ is stopped down to this diameter, so that the field of view is given for a pupil of this size. Care must also be taken that the eyepiece cup of the glass under test is brought into contact with the lens, as the field of view depends also upon the distance of the pupil from the eye lens.

In order to determine the illumination of the field of the glass, a very simple device is employed. Just above the lens of the observing camera a small sector-shaped window, W, is cut, in a tube in front of which are fixed a couple of ground glass discs and a glow lamp L′. A thin metal partition, M, is mounted in the camera, so as to divide off an approximately sectorial portion of the screen, which is illuminated from the window, W, instead of from the lens. Swivelling around the centre of the lens is a flat plate with pointer, P, and this is shaped so that a knife edge passes through one side of the tube and overlaps the window W. By turning the pointer, therefore, the area of the window is increased in proportion to the angle, and a scale of equal divisions on the cylindrical body of the camera C shows the percentage opening of the window. The small lamp L′ is run in series with the large lamp L, so that its intensity has an approximately constant ratio to that of the larger lamp; and it can be moved out and in until the illumination on the screen G, with the window W at full aperture, is the same as that from the collimator C, when no glass is interposed. When the field glass is placed in position, the magnified field is then found to be less bright than the sector illuminated from W, and the pointer P is moved, reducing the aperture of W until balance is restored. The reading on the scale then gives directly the percentage illumination of the field of the glass for a pupil of 5 mm. diameter.

The apparatus can be used also for measuring the absorption of dark glasses, negatives, etc., but for accurate measurements of this kind the absorption photometer to be next described is preferable.

### 5. Absorption Photometer.

The preceding instrument, though sufficient for ordinary commercial purposes, does not pretend to a high degree of accuracy so far as the absorption or illumination measurement is concerned, and the apparatus now to be described is intended for more refined measurements of this character. The principle of it was also described at the Optical Convention of 1905, although the arrangement has been slightly simplified. It consists of a wooden base about 3ft. 6in. long, at the ends of which are fixed two uprights bearing white paper or plaster screens, their centres being exactly one meter apart. These screens face one another, but are inclined at 75° to the line joining their centres, as shown in fig. 7. Between them a support carrying a small glow lamp L can slide along a scale S, so that the illumination on either screen can be increased relatively to that of the latter. In the middle of the front of the base is fixed a support carrying the observing device. This consists of an equilateral triangular box with three telescopic tubes projecting from the three faces. In the centre is fixed an ordinary 60° prism P, and it will be seen that light passing through the two tubes $T_1$ and $T_2$ is totally reflected from the two faces of this prism along the observing tube T. This device is so mounted that the tubes $T_1$ and $T_2$ point to the centre of

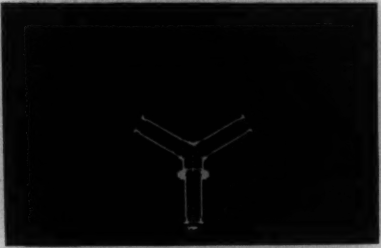

FIG. 7.

the two screens, so that on looking down the observing tube, these screens appear in close juxtaposition, the edge of the prism forming the line of separation. Similar lenses are mounted in the tubes, as shown, so that, if a telescope be interposed between one tube and the corresponding screen, its eye-point is transferred to the eyepiece of the observing tube. The whole of the device can be given a half turn about a horizontal axis. In making an observation, the apparatus is first set up without any telescope or other instrument, and it should be found that with the lamp L at the half-way point between the screens the two halves of the field appear identical. Should this not be the case, one of the screens may be slightly tilted to secure equality. The field glass or telescope or absorbing medium may then be placed in front of one of the tubes, and the corresponding half of the field will, of course, appear darker. By moving the lamp towards the screen viewed through the telescope, which can be done by the cord and pulleys, balance can be restored, and the percentage of light passing through the telescope or absorbing medium is

$$I = 100 \left(\frac{x}{L - x}\right)^2$$

where L is the distance between the screens, and $x$ the distance of the lamp from the nearer one. In order to facilitate the reading, a direct reading scale is marked on the bench, obtained from the formula $x = \frac{\sqrt{I}}{10 - \sqrt{I}} L$, so that the percentage of light transmitted can be read off on this scale when the balance is obtained.

The device is equally suitable for measuring the density of photographic negatives, and also for measuring the percentage of reflection from dull surfaces. In the latter case the surface to be tested is placed in contact with one of the screens, and the measurement taken in the ordinary way.

In order to make the apparatus suitable for measurements with coloured absorbing media or surfaces, a discrimination diagram can be slipped in front of the prism, and balance secured when equal facility of reading is observed on the two sides. A diaphragm or artificial pupil can be attached to the front of the observing tube when desired.

It goes without saying that for accurate work the readings may be checked, both by changing the position of the object to be tested to the other side, or by reversing the prism box.

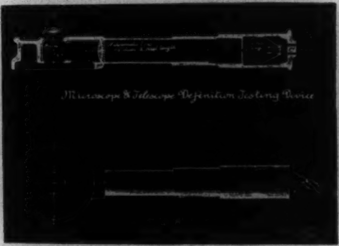

FIG. 8.

## 6. Definition Tester.

This simple little device is intended for the rough investigation of the optical qualities of a microscope or telescope, and for facilitating the adjustment of such instruments. It consists simply of a little cap (fig. 8), which can be dropped over the eye-piece of the telescope or microscope to be tested, and secured by three centering screws. Close to the eye there is a small aperture at one side, and a thin unsilvered plane mirror is fixed facing this aperture at 45°. If light from a distant small source passes horizontally through this aperture on to the unsilvered mirror, it is reflected vertically downwards along the axis of the microscope or telescope under test. If it then falls normally upon a mirror, it passes back through the instrument and through the unsilvered mirror to the eye, and, in the case of the microscope, if the mirror is at the focus of the system, the object appears sharp to the eye and unmagnified. Upon focusing the microscope up and down, the point of light enlarges into a disc, and the usual phenomena of racking a telescope in and out of focus upon a star are seen. In a perfect microscope, the disc should, of course, appear a white uniformly illuminated circular patch of light, but if it suffers from chromatic or spherical aberration, or astigmatism, this will not be the case, and the following well-known phenomena are observable :—

| | Inside focus. | Outside focus. |
|---|---|---|
| Chromatic Aberration | | |
| Undercorrected | Reddish margin | Blue margin |
| Overcorrected | Blue „ | Reddish „ |
| Spherica Aberration | | |
| Undercorrected | Bright edge | Bright centre |
| Overcorrected | Bright centre | Bright edge |
| Astigmatism or Tilting | Oval disc<br>Axis parallel to axis of cylinder | Oval disc<br>Axis perpendicular to axis of cylinder. |

The error of coma, of course, as was pointed out to me by Mr. Chalmers, is eliminated by this test, since the mirror reduces the system to a symmetrical combination. It might, however, be detected in many cases by permitting the light to fall upon some very small sharp point which scatters the light.

A convenient mirror in the case of a microscope is formed by the ordinary Abbé test plates, which are silvered slips underneath cover glasses of various thicknesses. The variation of defining power with thickness of cover glass, and with tube length is very clearly indicated by this device in many cases. In the case of a telescope all that is necessary is to have an optically worked plane mirror, which is placed square across the front of the objective. By rotating the cap, the image may be made to appear at the edge of the field, and the edge definition, apart from coma, observed. This gives a good test of the flatness of the field.

In order to save the necessity for a distant source of light, the device is also provided with a small side collimating tube with a pinhole aperture, glow lamp, and achromatic lens. This is sometimes more convenient, but it must not be forgotten that the errors of this small objective come into play. The aperture ratio of this lens, however, is not large, and where high power microscope objectives are under test, its errors will, as a rule, be negligible.

It should further be mentioned that this device enables the aperture of the objective to be estimated by the simple process of measuring the diameter of the disc of light resulting from a given alteration of the focus. This question will be gone into more thoroughly on another occasion.

In conclusion, I must once more express my regret that the apparatus has been made too recently to have given me the opportunity I had hoped to have of making measurements and giving results in this paper. But the preliminary trials which have been made with them appear to indicate that they fulfil their intended purposes very satisfactorily, and I hope to take another opportunity of bringing some results before you. I must express my most hearty thanks to Mr. Tinsley, of South Norwood, for having made up these instruments at such short notice, and for having put so much interest and energy into the matter. Thanks are also due to Mr. F. A. Darton for making one or two preliminary models, and to Mr. F. A. Burgess, B.Sc., for help with the illustrations.

# FATIGUE OBSERVATIONS.

BY DR. C. V. BURTON.

*Read 11th December, 1913.*

AN observer looking with close attention at the field of (say) a spectroscope or an interferometer experiences visual fatigue, which impairs his power of making accurate instrumental settings. The fatigue is no doubt largely psychological, especially when the illumination of the field is neither too great nor too feeble for comfort. It is then found that the most rapid relief, with restored power of accurate setting, is not afforded by cutting off all light from the eye, but by fixing the attention closely upon an object in which the contrasts are differently arranged.

As an example, I will take the case of a Michelson interferometer, this being the form of instrument with which I have had the best opportunity of making practical tests.* The field consists of concentric circular fringes alternately brighter and darker, and the reading-mark may be a very acute needle point seen in focus with the fringes; the whole being viewed through an eye-piece in the usual way. A setting consists in causing the fringe-system to open out or contract until the maximum blackness of a certain fringe passes through the needle point. For definiteness, suppose that the needle is placed with its axis vertical, its point being somewhere on a horizontal diameter of the fringe-system. Then the portions of the fringes with which the eye is concerned run in a vertical direction, the contrasts between them being distributed right and left. The observer, having become fatigued through fixing his attention on these contrasts, should now direct his eye to the highest or lowest part of the field, where the direction of the fringes is horizontal and the contrasts are distributed vertically. My experience is that in these circumstances recovery is rapid. The *retina* evidently is not rested, but the ill effect of concentrating the *attention* continually on one set of contrasts may thus be removed, very largely, at least, in the course of about ten seconds. It will be interesting to hear whether this observation is confirmed by others.

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* In the course of observations made in collaboration with the Earl of Berkeley.

In certain classes of work, as in (visual) spectroscopy, or in the measurement of spectrograms, it might be useful to have an eye-piece by the rotation of which the field could (at will) be turned through a right angle, so as to afford relief to the observer.

**Mr. Chalmers** said he hardly agreed with Dr. Burton's opinion that the effect was an entirely psychological one. He thought it was explained by the fact that the changed distribution of the dark and light bands might facilitate the recovery of the eye. He suggested that any modifications of the bright and dark parts of the field would tend to allow the purple to come back again into the parts of the eye from which it had been withdrawn, and so tend to make the eye in a fit state to receive any observations much more readily than leaving the eye viewing a purely dark field. He had had no opportunity himself of confirming the actual observations described by Dr. Burton.

**Mr. Baker** said he could confirm what Dr. Burton had related with regard to eye fatigue in that particular type of observation. A very short time of making such observations tended to fatigue the eye, and the accuracy with which observations of that sort could be made was certainly very much impaired after a minute or two of continuous work of that kind; but any attempt to rest the eye in the manner suggested in the paper was quite new to him; and was exceedingly interesting. He should certainly make a point of trying whether a similar method of resting could be applied to other kinds of observations.

### OCULAR ASTIGMATISM.

**MR. T. F. CONNOLLY** was afraid he had not sufficient knowledge of the subject to enable him to speak upon it with confidence, though it seemed perfectly feasible, psychologically and scientifically, that rest might be obtained for the eye in the manner suggested. It had occurred to his mind that the trouble might have something to do with residual astigmatism; that was to say, concentrated attention might induce a certain amount of astigmatic effect or compensation into the eye, and that might be relieved by a right-angled observation of the horizontal lines.

**MR. H. FIRTH** said if there was any truth in the theory of colour vision, then the visual substance was assimilated by the action of one kind of light, and desimilated by the complementary colour. Therefore it was possible these observations were rather in favour of the Hering hypothesis. He only threw that out as a suggestion.

**MR. CHALMERS** said the observations were made with absolutely monochromatic light, with dark and light fringes

**MR. H. FIRTH**: Then that would not explain it.

**MR. DIXEY** understood that Dr. Burton's observations were made with monochromatic light, and also that they were monocular and not binocular observations. That limited, not the value of the observations, but the consideration of them. The whole question of fatigue had many points of interest to a student of ophthalmological practice, and there were a great many points still to be threshed out. There were many forms of fatigue they were all familiar with. The ordinary luminous fatigue was quite a common phenomenon which the man in the street understood, at any rate, as regarded the practical application of it. There were questions with regard to the correction of anomalies of sight in their relation to fatigue which required a great deal more study and a great deal more elucidation, and suggestions such as those contained in Dr. Burton's paper were a valuable help to the study of the subject.

### A CRUCIAL EXPERIMENT.

**MR. CHALMERS** said he ought to have mentioned that when Dr. Burton originally raised the question he (Mr. Chalmers) had suggested to him to try and prove whether these effects were purely psychological by seeing whether relief could be obtained by looking with the other eye at the other pattern; but he was not sure whether Dr. Burton had had an opportunity of doing this.

**MR. IRVINE AITCHISON** pointed out that if the lines were constantly moving across the retina all the time, it must be specially difficult to obtain a focus at the same spot.

**MR. CHALMERS** replied that Dr. Burton had taken particular care to take his observations at a particular instant.

**MR. CONNOLLY** observed that the astigmatic suggestion of Dr. Burton seemed quite a feasible one.

**MR. CHALMERS** agreed, but was inclined to think that if there was a small amount of astigmatism present there might be a small amount of accommodation specially exercised for the purpose of getting over the fact that there was astigmatism present.

**MR. CONNOLLY** explained that what he meant was that if there was accommodation to neutralise the existing astigmatism, however produced, the relief would come at once.

**MR. CHALMERS** said he had suggested to Dr. Burton that he should consider the question whether it was characteristic of one position only, and he seemed to think the same effect was produced in whatever positions the pattern was observed. It was equally true whether the pattern was vertical or horizontal. Dr. Burton had not said definitely that it was so, but that that was his recollection, and he had expressed his intention of verifying it.

**Mr. Dixey** suggested that it would be valuable if Dr. Burton tried what would be the effect with other observers.

**Mr. Connolly** said the thing could be done perfectly well by simply viewing contrast lines with the most elementary apparatus.

# COLLECTIVE OR CO-OPERATIVE ADVERTISING FOR OPTICIANS.

BY H. F. PURSER.

*Read 11th December, 1913.*

THE idea conveyed by the title of my paper has been in my mind for years, and the present attempt to develop the subject is due, not so much to the creation of a successful scheme, but rather to the persistency with which the idea has been forced upon me by commercial experience, and the realisation of the fact that however difficult it may be to evolve a method of procedure now, it will be much more difficult in the future, unless the idea is accepted and recognised as one of the planks in the platform of optical trade politics.

It may be urged by some that since the Optical Society has by tacit agreement ceased to concern itself with trade politics and confined its attention almost entirely to the scientific side of the industry, the subject is hardly a suitable one for its consideration.

I plead, however, that as the proposition involves the education of the people by the scientist, and as the development of the industry, both in the scientific and commercial departments is very essential, it is a matter of deep concern to the Optical Society.

Now most of you are aware that although the optical industry embraces a very large number of articles of general utility, the total result expressed in pounds, shillings and pence is comparatively small.

We are accustomed to look upon the development that has taken place during the past fifteen years as very great, yet one has only to look around and compare the results with other industries to realise that the progress made is almost entirely confined to the ophthalmological department, whilst other departments have as a consequence been neglected. It is interesting to note, however, that the progress referred to is the result solely of education—first of the optician, and secondly of the general public, and that the method of education adopted for the general public has been that of advertising.

It is safe to say that the liberty possessed by the optician to educate the public by means of advertisement in matters pertaining to the welfare of the eyesight, has been productive of an enormous amount of good, and that the influence of every advertisement is such as to affect not only the individual advertiser, but every optician and oculist throughout the country. So that even here, although we have not collective or co-operative advertising, we have the effect of it.

Now, in the prescribing and fitting of spectacles and eyeglasses, the personal element will always hold a high place, for success depends on experience, knowledge, and tact; yet there are enormous possibilities in this department for co-operative advertising as distinguished from personal advertising.

The collective or co-operative advertisement I have in mind, and which I specially wish to call your attention to this evening, is one of many. Perhaps the most remarkable example is the modern system adopted by municipalities in advertising by means of posters, newspaper advertisements and booklets, the charm and pleasures of their respective towns as holiday resorts. In such cases the majority of the residents profit by the advertisement, and consequently have the privilege of paying their share of the cost through the rates.

The advertisements of proprietary goods may also be classed as a co-operative system, for although the retailer in such cases does not contribute directly to the cost of the advertisement he does so indirectly.

And here it may be advisable to say a few words on the merit and demerit of proprietary advertising in connection with the optical trade, because there are undoubtedly a large number of people who view it as a retrograde movement, and look upon it with grave suspicion. The effect of public advertisement, however, cannot be denied, and the large manufacturers, as owners of proprietary articles, are clearly in a much stronger position to advertise in the public press, which in these days reaches every home, and so places before the people a knowledge of things they could never otherwise have.

As examples in our own industry, we have the introduction and sale of the modern prism binocular. There can be no doubt whatever, in the mind of every man who has seriously thought of the matter, that the enormous quantity of these instruments sold, and the knowledge of their construction now possessed by the public, is due entirely to the fact that they were introduced and advertised by large manufacturing concerns, having at their disposal unlimited capital with which to educate the public. And in both these cases the retailer and the public have profited by the helping hand of the manufacturer.

Most of you have probably noticed during the past two or three years the very innocent advertisements in the form of paragraphs of reading matter, pointing out the nutritious value of the ordinary household currant, which in former days used to play "hide and seek" amongst the flour and suet, but which, we are now told, should be distributed in such quantities as to leave their presence no longer a matter of doubt, but a glorious and juicy certainty.

That paragraph is really an advertisement of an impersonal or co-operative nature. It does not proclaim the merits of Mr. Blank's currants: it deals only with the value of currants as an article of diet, and the effect of the advertisement is infinitely greater, and much more widespread, than would be the case of an advertisement of Mr. Blank's currants.

The educational value of this form of advertisement is of serious importance to the optical industry, which depends more than perhaps any other trade upon the knowledge and appreciation of the usefulness and necessity of the various articles offered for sale.

Notwithstanding the great publicity given to the spectacle business during the past few years, there still exists great prejudice and ignorance in the matter of spectacle wear, whilst the knowledge and understanding possessed by the public of the majority of articles contained in an optician's shop is exceedingly hazy, and as a considerable number of the articles in question concern seriously his daily life and health, one can conceive that it is possible by means of education to bring home to him the necessity and advantage of such things in the same way as has been done in the spectacle department.

Let us for a few moments refer to a few articles in an optician's store.

Probably not more than 5 per cent. of the general public know of the existence of such a valuable instrument as the lactometer, and a sale of half a million annually of these instruments is quite within the bounds of possibility providing the public can be educated to appreciate its value.

Although the same ignorance cannot be said to exist in reference to the thermometer and barometer, yet in these days, when hygiene and health receive so much attention, there should be much greater knowledge and appreciation of both these instruments.

Then we have the hygrometer, of which very little knowledge exists, excepting among those people interested in factories and mines where the Government have recently made it obligatory to use hygrometers. Bearing in mind, however, the effect of the atmosphere on the general health, and complexion, it should not be difficult by means of education to convince every man and woman that the hygrometer is an essential to health and beauty.

Coming to the more strictly optical articles, we may first consider the magnifying glass, which is capable of affording immense interest and knowledge of things in general. It is astonishing to find how many times in the course of a week one can make valuable use of a magnifying glass if carried in the pocket ready for use. The microscope is looked upon as the possession of the scientist alone, but its presence in the home would be a source of great interest and frequent usefulness.

You will probably have gathered from the foregoing remarks that the kind of advertising to which I specially refer in the title of my paper, is that which seeks by means of general information, in the form of complete articles and short paragraphs, published in the public Press and magazines, having as their object the education of the people in the need for, and uses of, the various articles included in an optician's stock.

All other schemes of co-operative or collective advertising may be dismissed as unpractical on account of their cost and the consequent difficulty in procuring the necessary funds.

It is, of course, quite well known to you that complete articles, extracts from lectures and speeches by scientific men, do appear from time to time in the public Press and magazines.

I have in mind a notable case, some years ago. Just before his departure from London to South Africa, Dr. Lindsay Johnson gave a lecture, the subject being, I believe, "The Eyes of Animals." I well remember that during a period of two or three months I read short paragraphs, evidently passed on from one newspaper to another in every part of the United Kingdom, and the nature of the extract was such as to be of great value to opticians.

'Science Siftings,' edited by our friend, Mr. Hyatt-Woolf, frequently contains short articles and paragraphs, educational in their nature, and, as such, very useful in the development of the optical industry.

The Spectacle Makers Company also does good work in this direction by the periodical publication of the particulars of their examinations and the work in raising the standard of optical efficiency amongst opticians.

With the exception of the last example, however, there is no guiding hand, and the publication of matter vitally affecting our industry is left to chance, so that even as regards the writings and speeches of our well-known scientists, many valuable opportunities are lost.

Imagine for a moment the possible advantages that would accrue to the industry if we could retain the services of a capable man whose duty it would be to extract from all possible sources, such as the speeches, lectures and writings of well-known scientists, and to himself write and secure publicity for such matter as would serve to educate the public in all things pertaining to our industry.

Supposing that coupled with the name of a well-known scientist you could secure universal publicity for such a simple statement as this: "If any difficulty is experienced in reading the printed matter thrown upon the screen at the picture theatre, it may be taken as a sign that the eyesight is defective, and that an oculist or optician should be consulted." Note, there are about 8,000 picture theatres, and consider how many people would be testing their sight.

Some time ago I had occasion to call in the services of two different firms of decorators at intervals of twelve months. In both cases the foreman in charge was a man between fifty and sixty years of age. Both times the work was badly done, and on discussing the matter very fully I found in each case the man was actually unable to see the defects complained of. The wearing of spectacles was barred as a sign of age and infirmity, each man assuring me that if he was seen with spectacles he would be discharged. The fact that such incidents are common knowledge amongst all those interested in the spectacle business, proves the need of still greater publicity.

Supposing, however, we could secure disinterested circulation of a statement addressed especially to masters and workmen, pointing out that the wearing of spectacles is not necessarily a sign of age, and that the efficiency of possibly 50 per cent. of the people would be increased by their use. The persuasive power of such a statement would surely be greater than the advertisement of any individual optician. Again, imagine for a moment an article written in a popular style pointing out the necessity and utility of a lactometer in every home, and suppose that extensive publicity is secured for it at the time the Rt. Hon. John Burns is bringing forth his Milk Bill in the House of Commons. Under such conditions I say without hesitation that a large business would follow, with a corresponding improvement in the quality of milk.

Perhaps at no time in the history of the optical trade have we needed a "champion" so much as during the "Titanic" enquiry, for the evidence then given on the subject of aids to vision at sea was such as to leave serious doubt in the minds of those who know anything at all about the subject, as to whether it was founded on gross ignorance or wanton distortion of facts, and one can well imagine that the impression left on the minds of the general public must have been very injurious to the sale of binoculars.

The "trade champion," if I may so designate the official whose appointment I propose as part of my scheme, would have seized the opportunity to contest the statements then made and prove that a satisfactory binocular as an "aid to vision" must be effective. For one moment remember that optics is more or less an exact science, and that in this respect it differs from most other industries in offering scope for what I call the impersonal advertisement, for the writer can nearly always treat of facts, and so influence the minds of the public, much more powerfully than the individual advertiser who, notwithstanding his statements of fact, is necessarily obliged to plead his own cause.

Now, however ineffectually I may have expressed my opinions, I am firmly convinced that there is enormous scope for work in the direction I have indicated, and that if our industry is to rise to the position necessary to maintain an Opto-Technical Institution, the time has arrived when we must close up our ranks and go forward as one United Body.

For the establishment and maintenance of an educational institution will involve considerable expenditure, and the necessity of finding employment for the students who are educated therein.

I think I have already said sufficient to satisfy you that there is still great possibility for the further development of our industry, both as regards the supply of our present needs and the creation of still more optical wants, and as one of the most powerful factors, I plead the cause of collective advertising, because it is effective, and its cost within reasonable limits.

My proposal is that we retain the services of a well-known all-round optical scientist, one whose knowledge of all the branches of our industry would enable him to write articles and extracts from all sources, such as speeches and lectures, matters of interest to opticians and influence the publication thereof in the public Press as matters of general interest, and on all possible occasions to champion the cause of optics and opticians.

As the work would, of course, only occupy a small portion of a man's time, the annual fee need not exceed, say, £250.

In arriving at this figure I am presuming that the literary matter will be published free of cost as information of general public interest, and that in some cases it may be possible even for the writer to receive payment from the Press. As to how far my presumption would be justified in practice perhaps some of our friends who have a more intimate acquaintance with the Press will be able to enlighten us. I am confident, however, that something could be done by a qualified champion on the lines suggested, to educate the public in the knowledge of things optical, so that the coming generation of opticians may have the necessary scope for their skill and energy.

So far my task has been comparatively easy. but now we come to the consideration of the source of income to provide for the appointment of a champion. I have to frankly confess to you that I am, and have been all the time, in trouble. The proposal made to you would have been given publicity years ago had it been possible to put forward a financial arrangement that could be easily worked.

If I were Chancellor of the Exchequer, and could levy taxes on all opticians to provide benefits for the craft as a whole, I should not hesitate a moment to impose a tax with the firm conviction that it would insure even more than "ninepence for fourpence."

It is a lamentable fact that notwithstanding the growth of the fraternal spirit, and the existence of numerous societies having for their object the betterment of the industry, there is an entire absence of union and a consequent enormous waste of energy and loss of opportunity for good. And whenever one comes up against questions involving finance—whether it is the seeking of parliamentary recognition, the support of educational classes, or the provision of an optical scholarship—one realises the utter hopelessness of the position, without that union which alone can accomplish so much at such a comparatively trifling cost both in time and money.

It may not be out of place to remind you of what is perhaps the greatest and most notable example of the possibilities of trade unionism in the history of the world. I refer, of course, to the doctors and their connection with the Insurance Act. By standing together they were able, with the help of the partisan spirit characteristic of our political system, to dictate terms to the Government of the greatest country the world has known, and to secure for practically the whole of their members settled incomes, which in many cases are greatly in excess of that they formerly received.

I have yet to be convinced that parliamentary recognition will give all that is claimed by its enthusiastic advocates, for if it gives no greater protection to its members than is given to dentists, its value will not be great, but if it was the means of forcibly bringing every member of the trade into union under one flag, I would support it to the utmost of my power, for with such a union we could accomplish practically everything we wished.

Who shall accomplish this union, and where shall we begin? We have many powerful and flourishing societies, notably the Spectacle Makers Company, the Institute of Ophthalmic Opticians, and the B.O.A., each doing a good work within the limits of their rules and Memorandum of Association, which make it possible for them to admit only certificated members of the trade.

What we require is a still wider platform than that offered by any of the societies I have mentioned—a platform that will admit of all members of the industry, and that will have written in large letters above it:

"The greatest good for the greatest number."

During the writing of this paper I have been wondering whether we have amongst us a Judas

Macabæus—a man who could fire the enthusiasm of opticians throughout the length and breadth of the land, and bring together all the forces under one banner, and, further, whether we could not so alter the rules of this Society as to make it the rallying ground of all the optical interests.

We are already in a neutral position. "No certificate is needed—nothing but a knowledge of, and an interest in, the optical trade."

Imagine for a moment the enormous financial power given by a society of, say, 4,000 members with a subscription of even so small a sum as five shillings, a sum which would be within the means of every member of the trade.

The advertising scheme I have outlined would be quite a simple problem. Insurance against trade claims, the provision of a scholarship, medals, and prizes, could be amply and easily provided for, with less work and worry than it now takes us to raise £100.

I would speak, however, beyond the confines of this meeting—to the members of all societies interested in optical matters, and say whichever banner we are eventually gathered under, let care be taken in the construction of the rules, etc., to secure power to use the funds of the Union for all legitimate trade purposes, including that of the appointment of a trade champion.

Until that happy time, I fear we shall have to rely upon voluntary effort to secure the necessary funds, and the extent to which support is possible will doubtless be revealed in the discussion which I hope will be freely indulged in, not only at this meeting, but in the trade Press. And if, pending the creation of the strong trade union to which I have referred, the various societies are able to draw together sufficiently to provide a fund to found a Publicity Bureau or Trade Champion, working on the lines I have endeavoured to unfold, I am confident the result will be of greater value, not only to our industry but to each individual member thereof.

# NOTE ON MR. GIFFORD'S METHOD OF MEASURING REFRACTIVE INDICES.

By S. D. Chalmers, M.A.

*Read 8th January, 1914.*

In the measurement of standard refractive indices, Mr. Gifford has avoided the troublesome measurement of the angle of the prism by using a 60° prism polished on the three faces, and taking the mean value of the deviations produced at each angle as being the deviation produced by a prism of exactly 60°.

The purpose of this note is to point out that each of the three angles of such a prism can be readily obtained by the auto-collimating method described by the author and Mr. Ryland to the Optical Society.

In this method the auto-collimating telescope is used to direct a beam of nearly parallel light normally to one face, A B, of the prism. Part of the light is reflected. The transmitted beam is divided into two; one part, after being totally reflected at the face, B C, falls nearly normally on the other face, C A, and is reflected back nearly along its own path to form an image in the auto-collimating telescope, nearly coincident with the image obtained by reflection at the front face. The observed difference in direction of these two beams is 2 $n$ times difference between the angles A C B and C B A; by observing the other beam we get 2 $n$ times the difference between the angles A C B and C A B, and hence the three angles of the prism. ($n$ is the refractive index.)

As the differences to be measured are small we can obtain the angles of the prism with a very high degree of accuracy.

Incidentally the goodness of the surfaces is subjected to a severe test, since the angle of incidence for the total reflection is about 60° and any sphericity shows itself in marked astigmatism in the image. By displacing the prism we can easily verify that the whole of each reflecting face is plane.

The knowledge of the angles of the prism, though not essential to Mr. Gifford's method, is very useful, in that the accuracy of each individual reading, as compared with the mean of three, is at once given.

In reply to a question put by a member as to whether, with the method in question, any errors in the refractive indices would not become more apparent and more disturbing than by the usual methods,

MR. T. SMITH said he thought it must be taken for granted that in the specimens measured by Mr. Gifford the refractive index did not vary appreciably; otherwise his results would have very little meaning. He believed Mr. Gifford had measured his refractive indices to something like six or seven places. At any rate, all the interest of the method chiefly lay in that the measurements, on the assumption that the material was homogeneous, could be taken to quite a high order of accuracy.

# (1) THE PAGET ARTIFICIAL HORIZON FOR ATTACHING TO THE SEXTANT AND USE IN A SEA-WAY.

# (2) THE BLISH PRISM.

By H. A. Hughes.

*Read 8th January, 1914.*

The Paget artificial horizon, as now made in its improved form, possesses many advantages over the various swinging and balance horizons at present used. It is not affected by vibration, wind does not disturb it, and any of the sextant telescopes can be used with it, thereby enabling an observation of greater precision to be obtained. It can be used to great advantage at night for stellar altitudes, a small electric lamp with pocket battery being provided for the purpose of illuminating it. It is readily attached to the sextant by a single thumb-screw, and can be quickly removed.

In construction of the instrument is a short, curved spirit-level tube, mounted in a brass tube with a prism above to throw the image of the bubble into and cause it to appear to move vertically up and down the field of the telescope through the clear half of the horizon glass. A lens in an adjustable tube in front of the prism throws the bubble into focus with whatever telescope is used. A small hinged reflector at the bottom of the horizon illuminates the bubble in daytime, and the electric lamp is used at night. Three fine cross or horizon wires are fixed across the level-tube, and an adjusting screw is provided at one end of the level tube with which the bubble can be compressed or enlarged to just occupy the space between the upper and lower wires. The instrument is fitted with adjusting screws in the event of its being injured or put out of order. A spare level tube is also sent, and can be readily fitted when required.

### To Use the Horizon.

Fix it securely on the sextant with the clamp screw, focus and screw in telescope to be used. See the bubble and wires focus sharp, angle the reflector at bottom of tube, so that the level is illuminated, or, if at night, attach the electric lamp and accumulators. See the bubble is the correct size (the space between the two wires), or adjust it to this by the screw.

Hold the sextant approximately level, and bring the sun or star down into the centre of field of telescope; then concentrate your attention on holding the sextant perfectly steady for a second or two with the bubble exactly between the upper and lower wires, and if the sun or star is not in contact on the centre wire or horizon, move the tangent screw what you think sufficient to make contact and steady again.

To test the instrument in clear weather by the sea horizon:—The sextant being in the usual adjustment and the horizon attached, set and clamp the index to zero, then hold the sextant so that the centre horizon wire and the sea horizon are in a line together; the bubble should rest in contact on the lower wire; if it does not, turn the tangent screw so that the sea horizon and horizon wire coincide, with the bubble resting in contact on the lower wire.

The reading on the sextant then gives the error, which must be corrected for the dip and refraction, and then applied as a constant error in all subsequent observations.

### The Blish Prism.

Captain Lecky says, in his "Wrinkles of Navigation":—"Owing to the uncertainty of the effects of refraction, the apparent position of the sea horizon can never be depended upon. It is found to be sometimes above its normal place, and at others below it. This displacement of the horizon sometimes occurs to a most serious extent."

In order to eliminate this error as far as possible, Commander Blish, of the U.S. Navy, designed an attachment to the sextant for observing back and front horizon simultaneously, and thus eliminating the error. It consists of a long rectangular prism with the end two faces bevelled off at 45° with the long axes at right angles with each other. A portion at each end of the flat side is polished. This prism is attached by means of a casing to the hollow leg or false centre of the sextant in such a manner that one polished flat face is directly opposite the top of the index mirror, and the other polished flat face is looking over the observer's head, the prism being made long enough to do so comfortably. With the index set at 0° the observer looks directly at the sea horizon in front of him, and at the same time sees the back horizon inverted, reflected by the prism. The two horizons are separated by twice the dip. When the horizons are brought into line, the corrected reading of the sextant is twice the angle of the dip.

The prism is very accurately ground and polished, as its value depends entirely on the angles being exactly right angles. The prism can be tested at the National Physical Laboratory and its error given.

## Discussion.

Mr. T. Y. Baker said he could endorse everything Mr. Hughes had said about the advantage which would accrue from the use of a chronograph fitted to a sextant. In taking observations at sea, one invariably took at least three observations, on account of the difficulty in making accurate contact between the limb of the sun or star and the horizon, and the mean of the results of the three observations was used for the purpose of calculation. It seemed to him, therefore, that if chronographs fitted to sextants were going to be of real use, there ought to be, as Mr. Hughes had more or less suggested, at least three chronographs in order that the observer might make his three observations. He thought the advantage which would arise from the use of one would not counteract the disadvantage of taking only a single observation in place of the three which were customary. But if it were possible to fit a self-recording chronograph with which a whole series of observations could be made one after the other, that would be a greater advantage still, especially for surveying work on shore. Where accuracy of seconds of arc was required, it was customary to take a whole series of observations, generally up to 20 or 30, at equal intervals of time, and a chronograph which could be used by the observer to take his own times, instead of having them taken by another person, would be of very great advantage indeed. With regard to the second instrument, the Blisch prism for measuring the dip of the horizon, that was an instrument which had been needed for a very great number of years, and he was rather surprised it had not been taken up before. If he remembered rightly, the prism was patented by Commander Blish ten or twelve years ago, and this was the first time he had ever seen one fitted to a sextant at all. The object which it served was the determination of the depression of the sea horizon below the true horizontal level. Of course, at sea it was necessary to measure observations from the sun or star down to the actual visible horizon, and that visible horizon was always a little below the true horizontal level, owing to the height of the observer's eye above the surface of the sea, and owing also to a certain amount of atmospheric refraction which tended to throw the horizon up. Now that atmospheric refraction co-efficient was by no means a constant amount, and the nautical tables were often seriously out, the errors amounting very often to from 8′ to 10′. About a month ago he read in 'Nature' a letter from someone who had been connected with the Egyptian Survey, where the difference between the actual visible horizon and the true horizontal line amounted to an error of 19′. Of course, an error of 19′ in the angle of observation would mean an error of 19 miles in the ship's position, which was a serious matter. The instrument before them seemed to him to fulfil the requisite conditions, and he was inclined to think that no sextant ought to be made without this attachment being fitted to it.

### THE SEXTANT CHRONOGRAPH.

**MR. W. SHACKLETON**, speaking of the chronograph, said there was an instrument, a sort of watch, about 2½in. diameter, which would record on the dial by pressing a button at the top. The handle of the chronograph carried a small phial of ink, and by pressing the needle at the top, the seconds were recorded in dots all round the dial. He had used that for taking transits of stars, and the last he had seen was one which was bought for the Antarctic expedition of the "Discovery," in 1900. That was all right in making observations in one minute, but if the time lasted over a minute, and more observations than one were being made in the minute, it was necessary to notice the minute and leave the record on the dial for giving the seconds. With regard to the prism which had been shewn, he did not know whether the Wollaston prism which he had in his mind was another thing entirely, or whether there were several Wollaston prisms.

**MR. HUGHES** said he was afraid he could not answer the point raised by Mr. Shackleton about the Wollaston prism, although he knew something of the work done by Wollaston. They in the trade always called that a Wollaston prism, though it was sometimes called also a double-star prism. He was grateful to Mr. Baker for the way in which he had explained—much better than he, the speaker, had done—the way in which these improvements in sextants were used. Mr. Baker had explained the matter from the navigator's point of view, while he had, of course, spoken as an instrument maker. Many officers had spoken to him of the difficulties in using the sextant as at present constructed, but whether in all cases they would be able to use the Blish prism he did not know. It had always been spoken of as a little bit of finesse in sextants, and, of course, it was expensive. For one thing, the prism had to be worked in one solid block, and it was impossible to treat more than three or four prisms in one plate. They were very troublesome indeed, and their weight tended to spoil their accuracy. He thought it would be a good thing, as Mr. Baker had said, to have these improvements fitted to every sextant—in fact, there were a lot of things which they, as manufacturers, would like to see fitted to sextants—(laughter)—but, unfortunately, officers would not take the trouble to do it.

**Mr. Shackleton** asked whether a 2-lens prism had been tried instead of the solid prism.

**Mr. Hughes** replied that he had tried two prisms, but the experiment did not answer. In making that prism there would always be some small error—something under a minute—and it would have to be applied to the mirror before using it.

**Mr. Baker** was of opinion that what Mr. Hughes had said as to the necessity of having a large prism was not absolutely correct. He had himself used a prism taken out of an ordinary binocular, quite a small thing, and had had no difficulty whatever, by just moving his head slightly sideways, in getting a view of the back horizon. The way in which the prism described to them was made was very excellent, enabling, as it did, the observer to take his observation right over the top of his head, but it was not absolutely necessary; it could be done with a very much smaller prism.

### THE WOLLASTON PRISM.

**MR. FAIRBANKS** said that one of Wollaston's best improvements in prisms consisted of making the hypothenuse surface of a right-angled prism in two parts, so as to correct the inverted image. The principle was seen in a camera-lucida prism. Instead of having a right-angle prism for the microscope, where the image was not erect, he used two surfaces instead of one, and thereby erected his image. That was Wollaston's improvement in the prism.

**MR. FIRTH** said the only Wollaston prism he knew was that fitted into the Gabriel thermometer. That prism gave a double image, and probably the instrument just shewn to them was called a Wollaston prism because it gave a double image.

**MR. T. SMITH** said it struck him, from an examination of the fittings Mr. Hughes had shewn them, that they were probably only intended to enable very rough angles to be measured. For instance, the bubble he had exhibited seemed to have a very considerable curvature, so that one would expect its sensitiveness to be comparatively small. He would like to ask Mr. Hughes whether that was done purposely, and if not, what was the reason for having so highly curved a bubble; also what kind of accuracy could be obtained from readings of the two fittings he had displayed. It also seemed to him, from a casual examination, that to obtain anything like accurate results with these instruments, the method by which they were attached to the frame of the sextant would have to be very carefully designed, so as to make sure that the instrument should always be in the correct position.

Mr. Hughes replied that the curvature of the bubble was done purposely. It was, in fact, a damping down; it was done to keep the bubble steady. It would be almost impossible for anyone to hold a bubble in one's hand on a ship with absolute steadiness, and, of course, it was found, when an attempt was made to use the bubble in an artificial horizon, that this damping down was necessary, as in the case of all pendulum instruments. Even in the case of the gyroscope, that had to be damped against the pitch of the ship. And it was the same with a bubble. In the case of the zinometer—the bubble with the index glass—that was only used for taking small angles, and it was found possible from the tables to get distances very accurately, within about 20 to 30 feet. It was very difficult to use the centre of the bubble at all, and, in connection with the artificial horizon, they had always used a curved bubble, because they wanted to make it usable, and if perfectly flat it could not be used. He was not sure that it would be possible to use a bubble curved to a less degree than the one he had exhibited, and, on the whole, from the experience he had had with officers of all kinds, he was pretty well satisfied that nothing more sensitive could be used. He considered it the closest thing, outside the gyroscope, to a fairly accurate artificial horizon that could be used, and if used as he had suggested, above the index glass, that would probably add a great deal to its accuracy. The important thing was the moment of "fix," when the star actually came to the horizon, and if that could be got accurately, the result would probably be to wipe out as much error as there was in having a curved bubble.

# SOME NOTES ON OPTICAL GLASS.

By S. Lamb, A.R.S.M.

*Read 12th March, 1914.*

It is not my intention to attempt to cover any more than a very small portion of the ground enclosed under the head of "Optical Glass," not only because the time at my disposal is so short, but minute details in connection with the subject would have no interest to many persons present. My proposal this evening is not to treat it purely from a scientific side, but to give an idea to the members of this Society not only of the method by which optical glass is manufactured, but the essential qualities of the material.

The uninitiated judge glass from one standpoint only, and this is the quality of colour freedom—and unless colour has been introduced for a particular purpose the result is in a great measure a reflection of the respective qualities. Ordinary tableware is in many cases very free from colour, and even window glass if viewed in thin sections does not show it to a pronounced degree. The best of our ordinary glassware if compared in strips of about 3′ long with optical glass such as B.S. Crown, fall deficient in respect of colour freedom, for this is one of the most important qualities of glass for optical purposes.

There is, however, another very important difference between the commoner articles of glass which may only be observed by a careful comparison. It will be seen that in ordinary glassware there are very fine silk-like threads, which may appear more pronounced in some samples than others, but whose presence would be a very serious item should the glass be used for anything except the cheapest optical purposes.

These fine silk-like threads are called striae, and may be seen readily in the bases of the ordinary tumbler—sometimes they may be detected in sheet glass intended for window glazing, even on looking through the two faces. By the methods of production of ordinary glassware these striae cannot be avoided, but it will be seen when describing optical glass production that special methods must be adopted to eliminate them from the finished product.

Window or glazing glass is one of the commonest because of its abundant use, and is produced from fluid glass by blowing, rolling, casting, or pressing. In each case, however, the aim is to obtain sheets of uniform thickness so that it will only be necessary for me to mention one of these processes to illustrate the method of manufacture.

FIG. I.—LONGITUDINAL SECTIONAL DIAGRAM OF TANK FURNACE.

### Window Glass.

The raw material from which this is made consists of essentially sand, lime, and a suitable form of alkali, with a small amount of other less important ingredients which constitute what is commonly known as Frit. With this is, however mixed pieces of broken glass which have accumulated during previous work. The furnaces in which such glass is made are of great capacity, holding as much as 400 tons of fluid material, and in general are heated by gas, being much hotter at one end than the other.

The raw material is introduced at one end (the hotter), where it melts, this being essentially the glass making end. The other end is known as the working end, being somewhat cooler, and from this place workmen are continually taking quantities for their manipulation.

Fig. 1 will illustrate this.

The furnace is thus continuous, and no attempt is made to perfectly mix the fluid glass so that layers of varying density exist from the top to the bottom of the fluid mass. In order to work the glass the workman gathers the requisite amount on an iron tube known as a pipe, and, with successive warmings, proceeds to "blow" what may be very much likened to a "soap bubble." The product is not, however, a sphere, but a cylinder with rounded ends, being thus formed by a combination of the internal air pressure and gravitational force. As an illustration of the working or blowing sheet glass the following illustrations will be useful. (Figs. Nos. 2 and 3.)

Fig. 2.—Early Stages, in the Formation of Cylinders for Sheet Glass.

Now it will be obvious that taking for granted that the glass is in layers of different densities these become wrapped one above the other as concentric spheres, all of which are expanded to lie like superimposed sheets. This will explain why sheet glass does not show a very striated appearance when viewed through its flat faces, but becomes very noticeable on edge viewing. This lack of homogeneity is not at all serious in such glass as sheet, but it is one of the most serious defects in optical glass.

Optical glass, as we know it to-day, is quite a modern product, and the names of many famous men are associated with the developments not only in the production of meltings on a small laboratory scale, but with improvements which have taken place in the art of manufacture. Of the earliest we may note such names as Fraunhofer, Guinand, Harcourt, Stokes, Hopkinson, Boutemp. The work of these along with numerous other investigators was not only to improve the methods by which means large pieces of homogeneous glass could be obtained, but to extend the series by means of small scale trials.

FIG. 3.—LATER STAGES IN SHEET GLASS BLOWING.

Stokes gives, in the British Association Reports, 1871-1874, an account of Harcourt's work, from which we learn that he produced 166 small meltings, but the great difficulty of rendering the glass sufficiently homogeneous to allow accurate spectroscopic determinations, did not appear to be overcome. Professor Abbé in 1876. after referring to Stokes' reports on Harcourt's work, laid the foundation stone of the subsequent important developments in optical glass. His remarks induced Dr. Schött to take up the question, and in 1881 a joint investigation was commenced, the ultimate results of which are known to the whole of the optical world.

The underlying principles of optical glass manufacture have not been altered in a marked degree, although the glasses available for use have been increased in number, and certain operations such as annealing have been much improved.

## Manufacture of Optical Glass.

The furnace used is comparatively speaking small, containing what is called a pot or crucible in which the glass is made and allowed to cool, being broken up when the melting is completed. (The lecturer here showed a small sample pot.)

The pot is built by hand from fireclay, whose plasticity has been rendered about equal to that of putty by the requisite water additions. It is allowed to dry very slowly for a period extending over at least six months in order to lose all water except that of combination. In this state it is fairly hard and can be handled provided undue roughness is avoided.

Assuming that a pot has been passed for use it is placed in a furnace known as a "pot arch" and its temperature gradually raised in order to remove the combined water and to burn the pot as hard as possible. During this operation the pot is raised to approximately as high a temperature as that of the glass-making furnace into which it is ultimately placed.

At the end of seven days the burning may be considered complete, when the red-hot pot is picked up by a machine and placed in the making furnace, and the aperture through which it is introduced is well bricked up and covered with fireclay. During the transference of the pot it is struck lightly with an iron bar, and when the sound emitted is clear and bell-like its further treatment is not attended with such anxiety as one whose note is very dull—the latter usually indicating unseen flaws.

After the pot has been placed in the furnace (fig. 4), it is allowed to remain for several hours, during which time it attains a bright red heat and the raw material is being prepared. This raw material, of course, depends on the nature of the glass under preparation, but in any case only the purest ingredients can be used in order that the colour may be maintained at its highest possible standard.

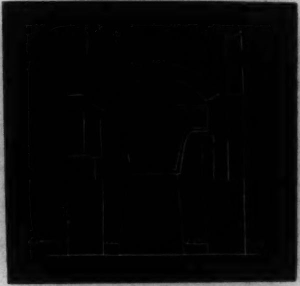

FIG. 4.

For flint glasses such materials as lead oxide, alkali and sand are used; and for crowns: sand, lime, oxide of barium, alkali, oxide of zinc, oxide of alumina, boric acid, etc., in amount depending on the optical constants required.

At the end of the warming period (6 hours) the glazing material, which consists of small pieces of glass from previous pots of similar glass, is introduced, and as soon as melted the inside of the pot is covered with the molten glass —an operation technically called "glazing."

After a further interval of about two hours the raw material (to which has been added some small pieces of broken glass) is introduced a little at a time, each "filling" being allowed to melt before another is added. This alternate adding of raw mixture and heating is continued until the pot is filled, when the heat of the furnace is raised in order to free the glass from bubbles. This operation is one of the most important, and the success or failure of any particular pot depends very much on the care taken and the judgment exercised during this period.

For every glass there is an important range at which successful working is possible—too low a temperature does not allow the bubbles to escape while too high a temperature results in the fireclay of which the pot is made being severely attacked, and defects appear which will be mentioned later. This temperature, which is different for different glasses, is maintained for several hours, during which time small trial pieces are withdrawn frequently to test the progress. When no bubbles are seen on the trials the operation is at an end, and the temperature is allowed to slightly fall during the next four hours, when the mouth of the pot is opened and any "scum" which has been carried to the surface on account of its lightness is taken off.

The glass at this stage should be free from all defects except that it is by no means homogeneous, and even small pieces would show an abundance of similar defects to those mentioned when speaking of commoner glass, *i.e.*, veins or striae.

## Removal of Striæ.

*Stirring.*—The aim of this operation is to remove these striae, and to Guinand is due the credit of first introducing the method by which such is possible. The "stirrer" consists of a rod of fireclay similar in composition to the pot in which the glass is made, being solid for its whole length except a small recess at one end in which a curved metal rod can be fitted. This rod is continued over a swivel wheel situated close to the furnace, and ends in a suitable handle, by which means it is guided round and round the pot. (This was illustrated by lantern slide.)

The operation is continued from four to fifteen hours according to the glass, and is very trying to the men, who have to be changed at frequent intervals. The glass is, however, cooling down gradually, and when it becomes so viscous that the stirrer can only be moved with difficulty, it is removed, and the pot with its contents is picked up and transferred to an annealing oven, where it is allowed to cool.

During cooling the mass breaks up into innumerable pieces varying in size from the very smallest up to several hundredweights, which are carefully sorted over and defective pieces rejected: the largest being generally retained for preparing sizes above the usual requirements, and to these we shall have cause to refer again.

The smaller pieces are subjected to the next operation in order to prepare such articles as plates, prisms, discs, etc., the individual weight of which does not exceed a few pounds.

*Moulding.*—All the favourable rough lump glass is subjected to this operation, which consists of gradually raising the temperature until it becomes plastic, and then by suitable tools giving to the pieces the shape required. As each article is moulded it is passed into a separate kiln, where the final annealing is carried out extending over a period of about 10 days for ordinary sized plates.

The final annealing process increases according to the size and shape of the objects in hand—in fact in some cases it is spread over a period of several months.

*Examining the Glass.*—The glass cannot be satisfactorily examined until the whole of the forementioned operations have been completed. Plates are polished on two opposite edges, small discs on four facets, prisms on two or more flat surfaces, in order to select the good material from the defective. Very much glass is at this point found unsaleable, in fact a yield of 20 per cent. from any particular pot is considered satisfactory.

The defects which cause rejection may be divided into two classes: (1) Those which cannot be removed by a subsequent operation, *i.e.*, bubbles (sometimes called seed), striae, stones. (2) Those which can be removed by a subsequent operation—stresses.

## CLASS I.

*Bubbles.*—The interactions which take place during the melting operations in some cases liberate as much as two cwts. of gas per pot of glass, so that no further explanation is needed to indicate the source of this defect, and although the bulk of this is readily removed, yet the last traces are often troublesome.

Now, in order to perfectly free the glass from bubbles, it is essential to raise the temperature above a certain minimum, when the combined effect of fluidity and gaseous expansion renders it possible. Certain of the glasses (notably those containing large quantities of barium oxide) appear to be impossible of production free from fine seed, which has been explained by assuming that the glass at high temperatures continues to give off bubbles. To these special cases the optician and his client have become accustomed, but excluding these the demand is in general for perfectly seed-free material, and I have no doubt the optician is guided more or less by his clients' objection to the appearance of seed, imagining it to be a very serious defect, but when one takes into account that even in extreme cases the loss of light only amounts to 1/5,000th it becomes practically negligible.

Perfectly bubble-free glass does not of necessity mean a better glass except in appearance, because the high temperature needed facilitates the attack on the fireclay of the pot, which increases the impurities taken up, thus increasing the light absorption value slightly, and rendering the perfect mixing by stirring more difficult.

This latter point would suggest that in bubble-free glass there is a greater possibility for cases of want of homogeneity to occur—a point that is worthy of consideration.

*Striae* (*Veins*).—This defect is produced during the melting operation, and is the cause which necessitates the rejection of the major portion of the glass made. The two terms are used to describe the same defect, although the term "veins" is generally reserved to describe heavy striae. The most important source of striae is the action of the fluid glass on the fireclay pot during melting, being threads rich in silicate of alumina, although volatilisation of certain materials during the time the glass is maintained at a high temperature is a contributing source. Borate glasses are prone to carry striae as boric acid is freely volatilised from glasses which contain it. This loss of a constituent at a particular point produces layers of slightly varying refraction, which can only be overcome and completely incorporated by stirring.

The presence of striae is described as want of homogeneity, which is undoubtedly correct, but this term is more comprehensive when used by the optician. He calls striae "striae," or may be "veins," but reserves the term "want of homogeneity" for something less than visible striae, which produces a deformation of image. It is probably caused by a gradual index change due to the fact that although the striae have been broken, they are invisible, yet have not quite reached the perfect state of incorporation.

Very heavy striae may be easily seen by the unaided eye. Finer striae may be detected by a method shown in fig. 5.

"Want of homogeneity," which is somewhat rare, can only be detected when other possible causes, such as curves, stresses, etc., have been eliminated.

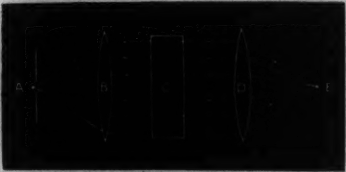

FIG. 5.—DIAGRAM OF STRIAE TESTING APPARATUS.

The method by which these defects may be minimised is by adequate stirring, but on account of their method of formation it is doubtful whether they can be entirely eliminated.

*Stones or solid bodies.*—These do not in a great measure trouble the optician owing to the fact that they are avoided as far as possible, and portions containing them rejected during the sorting operation.

They may consist of undissolved particles, but more frequently result from small pieces of clay leaving the pot and remaining suspended in the glass when too high a temperature has been reached. They continue to dissolve as they are in suspension, but this process is very slow, due in a measure to the viscous envelope which surrounds them. During stirring this envelope becomes elongated, and from each stone a tail-like filament identical with a vein is produced. This defect is very serious, and whole pots of glass are rendered useless should it occur to any extent.

## Class II.

The effect of stress on glass has been fully investigated by Pockels proving that the change of index with uniform compression increases rapidly with the original density and index. Glass is a poor conductor of heat, and if such a body is cooled down from above its softening point rapidly, *stresses* are induced, depending on the rate of cooling. The body first solidifies on the outside, and being very rigid, will not allow the hot fluid interior to contract to its full amount, thus such a system should be under compression near the sides and tension at the centre, and a varying refraction must result. I have here two glass spheres about 4-in. diameter. They were made from fluid glass, but in the first case it was chilled by allowing to cool as rapidly as possible consistent with keeping it intact, while the other has been carefully annealed. The bubbles in the unannealed glass are seen to be very large in the centre, gradually fading away towards the outer edge. In the annealed sample the bubbles are very small and of uniform size—about 1 mm. in diameter.

No glass is perfectly free from strain, but that found in the highest qualities of optical glass is of negligible amount. (Here the lecturer showed samples of good and bad annealing.)

Glass in general is a very complicated substance, and it would be a difficult matter to assign a chemical formula to express its constitution. It, however, behaves so much like a solution that it must be considered as such certainly in most cases. There is, however, a tendency in all glasses to depart from their glassy nature, depending on the chemical composition.

Most, if not all, of the glasses, if maintained just above their softening points for a considerable time, would lose their glassy nature and become very similar to porcelain, and advantage is taken of this fact in some cases.

This phenomenon is known as "devitrification," and opinions differ as to its exact nature; some consider the crystals to have an identical composition with the glass, while others conceive it to be a true crystallisation of a chemical substance from the solution. As regards myself, I take the latter view. With the ordinary crown and flint glasses this tendency to devitrify is not very serious, but as the optical constants become what may be termed abnormal, the chemical composition is altered to meet the case.

The alteration of composition along a line generally means increasing the amount of one or more of the constituents in the solution and the increasing concentration ultimately reaches a limit when the glass cannot be prevented from depositing crystals. This tendency to devitrification is in many cases the phenomenon which prevents what may appear a very useful series of glasses from being realised in practice.

Examples of devitrified material do not often appear in public, but I have a few examples which may be interesting to a number present. Some very interesting samples were here shown.

## Large Blanks.

Earlier this evening, when speaking of the glass from a pot, I mentioned that the larger pieces were generally retained for work of unusual dimensions, and among these may be mentioned telescope discs, prisms, or large plates.

The first stage in preparation is essentially the selection of a suitable lump of volume well above that needed for the finished product; in fact, two or three times as large, and before any work is justified no excessive number of defects should be seen in the rough blocks. This operation alone will usually eliminate a large number of blocks from undergoing any further work in the intended direction.

Assuming for a moment that a block has been selected, it is first essential to polish this on four faces at least in order to select any portion which may be defective for removal. Should any portion be found useless—and there are few large blocks in which this does not occur—it is as far as possible cut away.

The block is now subjected exactly as in the case of the smaller work to a moulding operation, either into a rectangular or octagonal plate, and again subjected to examination by polishing its edges. Any defects are again removed, and the moulding operation performed, followed by another examination. These may have to be repeated four or five times, and the risks of breakage not only in heating, but in subsequent cooling, are very great.

As soon as all the defects have been eliminated by successive grindings, the glass is moulded to circular form of correct diameter and thickness, and finally subjected to a very careful annealing operation. It will be seen that the difficulties involved in not only obtaining large blocks but in their subsequent manipulations are very great.

For specifying the refractive properties of a glass it is customary to use bright spectral lines which can easily be obtained from artificial sources.

The lines which are generally adopted are the red potassium line, the yellow sodium line, and the lines $H\alpha$, $H\beta$, $H\gamma$ of the hydrogen spectrum. Of these the sodium, $H\alpha$ $H\beta$, are identical with the Fraunhofer lines D, C and F in the solar spectrum. The wave lengths of these in tenth meters may be taken as

| | |
|---|---|
| $A^1$ (potassium) ... ... | 7,677 |
| C (hydrogen) ... ... ... | 6,563 |
| D (sodium ... ... ... | 5,893 |
| F (hydrogen) ... ... ... | 4,862 |
| $G^1$ (hydrogen) ... ... ... | 4,341 |

The characteristic quantities $n_{A^1}$, $n_C$, $n_D$, $n_F$, $n_{G^1}$, are spectrometrically determined for each melting, and from them are deduced the mean dispersion $(n_F - n_C)$, and the partial dispersions $(n_D - n_C$, $n_F - n_D$, $n_{G^1} - n_F)$. One very important quantity is deduced from these figures, and is commonly known as $\nu$. Its value is expressed by $\frac{n_D - 1}{n_F - n_C}$ and is often called the dispersive reciprocal. being an important factor in lens systems.

The theoretical and practical optician is the important person who makes the demand which must be met by glass manufacturers, and although to-day such possibilities as increasing the range of optical glasses appears very small, yet we shall find that ultimately the production of what we now term abnormal glasses will be successfully accomplished.

# ON THE MEASUREMENT OF CURVATURE OF SMALL LENSES.

By S. D. Chalmers, M.A.

*Read 23rd April, 1914.*

The method described is applicable only to a polished surface, and is a modification of that used in the ophthalmometer.

An object $O\,O_1$ is placed in front of the reflecting surface, and an image, real or virtual, is formed at $I\,I_1$. Even though the object be too large for the simple theory to apply, we can still obtain the ratio $I\,I_1 : O\,O_1$, provided none of the reflected light is incident at too large an angle. We have

$$\frac{II_1}{OO_1} = \frac{CI}{IO} = \frac{IP}{PO} = 1 + \frac{2IP}{r}$$

also

$$\frac{OO_1}{II_1} = 1 - \frac{2OP}{r}$$

so that

$$\frac{II_1}{OO_1} - \frac{OO_1}{II_1} = 2\left(\frac{IP+OP}{r}\right)$$

so that if $I\,O$ be a known distance we have

$$r = \frac{2.IO}{\dfrac{II_1}{OO_1} - \dfrac{OO_1}{II_1}}$$

As the image $I\,I_1$ is virtual, it must be viewed in a microscope, and the magnification given by the microscope will be affected by any error in focusing.

To avoid this error the microscope is moved parallel to the line $O\,O_1$ till the image I comes on the cross line, and then moved across till the image $I_1$ comes o nthe cross line. The distance moved by the microscope is $I\,I_1$, and corresponds to the distance $O\,O_1$.

In order that the distance $I\,O$ shall be constant, it is desirable to attach the object to the microscope, and the length $O\,O_1$ will be the actual separation between the object points (at any time), and the distance moved by the microscope.

**Fig. 1.**

Provided the distance (*d*) between the object points is not too great we have, writing $I\,I_1 = x$,

$$2r = \frac{2IO}{\dfrac{x}{x+d} - \dfrac{x+d}{x}} = \frac{2IO\,\delta_1 x}{d} \times \frac{x+d}{2x+d}$$

When the radius is very small it is proportional to $x$; when it is somewhat larger the radius equals $x\dfrac{IO}{d} - \dfrac{x^2}{d}\,\dfrac{IO}{d}$ + terms involving $x^3$.

For these cases the values of the co-efficients of $x$ and $x^2$ could be obtained from the given data, but they are more readily obtained by calibrating with a convex and concave surface of known equal and opposite radii. This empirical calibration can be extended to the case where the object is large, but except when the radius is very small it is preferable* to use an exact formula for computing the radius.

In fig. 2 let the light reflected at P travel along the axis of the microscope as if it came

Fig 2.

from I, and denote the angles N C P, O I P, and P O I, by A, B and D respectively.

Then $2A = B + D$

and
$$r = \frac{PN}{\sin A} = \frac{x}{\sin \frac{B+D}{2}}$$

Since the exterior angle P of the triangle O P I is bisected by P C

$$\frac{CI}{CO} = \frac{PI}{PO} = \frac{\sin D}{\sin B}$$

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*I am indebted to Dr. G. F. C. Searle, F.R.S., for the suggestion that an exact method of calculation should be employed. The formula used differs from that suggested by him.

but $$\frac{CI}{CO} = \frac{IM}{OE} = \frac{x}{x+d}$$

*i.e.*, $$\sin D = \frac{x}{x+d} \sin B,$$

and $\sin B$ is $\frac{OD}{OI}$, and is constant for the apparatus.

MEASUREMENT FOR A PAIR OF TEST PLATES.

$2\,DO = 169.4$ mm. microscope with stage reading to .01 mm. by estimation and 50 mm. o.g., distance between object and image $OI = 240.0$.

| | Convex. | Concave. |
|---|---|---|
| separation of images $x$ | 9.40 | − 8.49 |
| $\sin B = \frac{84.7}{120}$ | | |
| $B = 20° 40'$ | $B = 20° 40'$ | $20° 40'$ |
| $\sin D = \frac{x}{x + 169.4} \cdot \sin B$ | $D = 1° \; 3'.8$ | $- 1° \; 4'$ |
| | $21° 43'.8$ | $19° 36'$ |
| | $\frac{B + D}{2} = 10° 51'.9$ | $9° 48'$ |
| $r = \frac{x}{2} \Big/ \sin \frac{B+D}{2} =$ | 24.94 | − 24.94 |

The value of the radius as determined on a spherometer was 24.96 ± .04.

The value of 240. was measured to the nearest .5 mm. only, and the values of $x$ to .01 mm. The limitation to the final accuracy seems to be the accuracy of measuring $x$.

The only practical difficulty that arises is due to the fact that with high-power objectives the mount of the objective is likely to cut off unless the object is taken sufficiently large.

The method is applicable to lenses which are too small to be measured on the spherometer, and has been applied to lenses of as small a radius as 1.8 mm.

# THE DESIGN OF A SEXTANT.

By T. Y. Baker, M.A.

*Read 23rd April, 1914.*

I PROPOSE to-night to lay before you a few points relating to the design of sextants which are of distinct importance to the user of the sextant, and therefore presumably to the makers.

It might be supposed that as sextants have been in use since the time of Hadley, some sort of finality of design would have been reached by this time, and in a sense that is true. It might also be supposed that this final pattern exhibits the nearest approach to perfection that sextant makers can attain. That is far from being the case; another reason that makers have not yet produced the best instrument possible is to be found in the fact that they themselves have never had occasion to use a sextant or to see one used. If they were to take even a short sea trip and try their hands at determining the ship's position by sextant observations, I think that much good would come out of their experience, and that we should see further improvements on vital points instead of so much time and thought being devoted to unimportant details.

Quite ninety per cent. of the sextants that are made are destined to be used for sea work only; that is to say, for the determination of latitude and longitude of a ship which is out of sight of land. There are, of course, many other purposes to which a sextant is put, especially in the Navy, but the bulk of the sextants made have no other mission in life than the one I have just stated. My suggestions and my criticisms tonight refer almost entirely to sextants of that sort, and must not be taken as equally applicable to the more elaborate instruments that have to be used for special purposes such as nautical surveying.

To most of you the science of navigation is one which is not very familiar, so I should like just to indicate what it is a navigator has to do to fix his ship's position. He measures the altitude of some heavenly body, sun, moon, or star, above the visible horizon, and at the instant of measurement takes the time by the ship's chronometers from which the Greenwich mean time of the observation can be determined. The observed altitude after sundry corrections is subtracted from 90°, and the true geocentric zenith distance is obtained. Let us suppose that this zenith distance is 50° 10′.

FIG. 1.

The true zenith distance for some place near the ship's position, say A, is then worked out for the same instant as the observation. That is a comparatively straightforward piece of spherical trigonometry. Let this calculated zenith distance be 50° 0′. The deduction that the navigator makes from his sight is that his ship is not at A but on some line C B D, which is drawn on the chart at right angles to the true bearing of the heavenly body observed, and 10′, *i.e.*, 10 sea miles, further away from the heavenly body than A is. In the figure A B = 10′.

You will appreciate from what I have said the connection between the errors of his observation and the resulting error of the position line C B D. If the altitude has been measured badly and is one minute out, the position line will be moved one mile parallel to itself.

The errors of the line of position are consequently identical with the errors of the observation, and since the suitability of the design of the sextant is closely connected with the resulting errors that the instrument will have, it is necessary to examine the errors of the observed zenith distance somewhat carefully. The calculation of the zenith distance for A can be made, if necessary, with an accuracy of fractions of a second, so that we may consider that the whole of the error of the position of C D lies in the error of the observed zenith distance.

This error may be expressed conveniently as the algebraic sum of three separate errors: the error of the observation, the error of the sextant, and the error of the corrections which are applied. I want to look at all these in detail for upon their respective values depends the whole question of the design of the sextant. Before doing so, however, I want to point out what I think is a matter of common sense, namely, that it is a sheer waste of time to endeavour by careful elaboration and infinite precaution to reduce one of the errors to the order of seconds if another may be several minutes. I am not suggesting that a slight improvement is not made by reducing any one of the three as low as possible, but the reduction in the final probable error is not comparable with the reduction that is made in the special error that was chosen.

Error of Observation.—The inaccuracy in this case depends upon the skill of the observer and the magnifying power of the telescope. Contact between a circle and a straight line cannot under the best circumstances be made with a greater accuracy than about half a minute at the eye. But the circumstances of a sextant observation are far from being the best. At the least motion of the hand both sun and horizon move in the field of view, and independently. The sun appears to swing in a circle with a centre somewhere above the visible horizon. [The figure is upside down, as an inverting telescope is generally used.]

The object of the observer is therefore to set the index bar of his sextant so that the sun just dips into the sea in the course of its swing (if the sight is in the morning), and then wait until the instant when the sun comes into contact with the horizon.

Fig. 2.

Particularly must he avoid making an observation in the position of the dotted circle. It seems to be in contact, but a small movement of the hand will swing the sun into the sea.

It is difficult to put an exact value to the error an observer will make, but I should think, in view of the fact that the horizon may be indistinct, the ship rolling and vibrating and making it consequently difficult to hold the sextant steadily, that a moderately skilled observer will make his observation never more than one minute out. I should hesitate over lowering this figure.

Much better results can be obtained in an artificial horizon where the contact of two bright circles is a much easier one to determine.

THE SEXTANT ERRORS.—These include the first order errors due to bad centering, bad cutting of the scale, bad shades and mirrors, and the index error (zero error); also second order errors due to want of perpendicularity between the two mirrors and the plane of the arc, and want of parallelisms between the axis of the telescope and the plane of the arc.

## The Cumulative Error.

I shall have to return to these later, so I will merely state that their cumulative value would ordinarily be a minute or so.

THE ERRORS OF THE CORRECTIONS.—The corrections of the observed altitude are index error, dip of sea horizon, semi-diameter, and refraction and parallax of the body in altitude. The error of the first correction is properly included in the sextant errors; the semi-diameter can be obtained with absolute accuracy, and the refraction and parallax correction if the body is sufficiently high in the heavens will agree very nearly with the values given in books of nautical tables. But the dip correction contains the greatest source of error in the whole science of navigation. Its inaccuracy depends entirely upon the variability of the refraction in the lower layers of the atmosphere. If one knew the laws of density and temperature in that part of the atmosphere between the surface of the water and the eye of the navigator, some thirty or forty feet above, one could determine the exact path of the ray of light which proceeding from the horizon ultimately reached the navigator's eye. But one does not know any such laws. The density and the temperature are constantly changing, and any formula for the dip that is given in nautical tables, even with the modern corrections for difference of temperature between sea and air, is most unreliable. One has only to compare the tables given by different authorities to realise how unreliable the values are.

The variation in the dip correction seems mainly to depend upon the temperature gradient between the surface of the water and the observer's eye. If the sea is warmer than the air the temperature will fall continuously between the two points, but not necessarily uniformly. On the other hand, if the sea is cooler than the air the temperature will first of all begin to rise as one goes upwards, will reach a maximum and afterwards fall again. This second case, which is quite a likely one to happen, is the one which affects the accuracy of the dip correction most.

I quote the following results from a book on atmospheric refraction, written by Mr. T. de Graaf Hunter, Mathematical Adviser to the Indian Survey, and published by the Indian Government. For a height of eye of 40 feet above the sea the dip of the visible horizon is 5′ 56″ if the temperatures of the sea and air are equal, and the latter is uniform between the sea and the observation point. If the sea is warmer than the air and the temperature falls in the air *uniformly*, so that there is a drop of 1° F. at the observer's eye, the correction for dip is 6′ 33″, *i.e.*, 37″ more. On the other hand, if the sea is colder and the temperature rises as one moves upwards at the rate of 1° F. for 15 feet, the correction for dip is zero, so that the tabulated value is nearly six minutes in error.

Mr. Hunter remarks that if the temperature gradient is of this value (1° F. for 15 feet), the earth appears flat, and with a steeper gradient it looks concave.

Statements of this sort are not mere theory. Perfectly authentic cases are known of a ship's position being as much as twenty miles out under circumstances which entirely preclude any great source of error except in the value of the dip. Particularly have errors of these large amounts been noted in the Red Sea and in the Antarctic. In 1899 the Austrian Navy made a great many experiments from Pola in the Adriatic, and as a result of their work they tabulated corrections to be applied to the dip under certain conditions of difference of temperature between the air and the sea. But they were careful to explain that these corrections could not be trusted unless a wind of at least two to three were blowing. They could not, in fact, be relied upon in the least on a still, calm day. The Austrian officers found on occasions that the dip correction was over nine minutes out.

Further, these experiments shewed that dip values differing from the tabulated values by 3′ and more were of very frequent occurrence.

Now, the customary practice of navigators is to take the tabulated values as being correct, without even applying the corrections for the barometric and thermometric heights. Such a practice is hardly defensible. It is true that no two compilers of the temperature corrections agreed as to what the values should be, but that is hardly a reason for taking out the tabulated value that is given to seconds of arc when it was known that errors of 10, 15, even 20, minutes were possible. But if the navigator's practice is at fault, so also is the sextant maker's design. It is perfectly futile for a maker to supply a sextant which will never be used for anything but sea work, that reads to 10″, unless he provides some method of measuring the dip of the horizon in conjunction with the instrument. Yet that is what is always done. The sextant is tested at Kew to determine its centering error. This error is given to the nearest 10″, and the ignorant mariner imagines that the sextant he buys can be relied on to that order of error.

I believe the retailer has been known to charge an extra guinea if the Kew error nowhere amounted to 10″. The fact is that if a sextant has no means of measuring the dip there is hardly any sense at all in cutting the arc finer than a couple of minutes, in view of the uncertainty of the dip correction. If there is such a fitting, a greater refinement may be aimed at, but I should think that in no case for ordinary navigation, as apart from surveying, or cable laying, is anything gained by making the sextant read finer than half a minute. I should be much more inclined to put it at one minute. The only method of measuring dip at sea by means of the sextant itself is by the agency of some fitting, such as the Blish prism, shewn at a previous meeting of this Society. That prism has been in use in America apparently since 1902, and by its aid Lieut. Blish has discovered many occasions of discrepancy between measured and tabulated values for the dip, amounting to as much as seven minutes in some cases. To provide a sextant for sea work without a dip meter is about as sensible as it would be to leave out the lower scale divisions, so that the zero error could be computed.

It may perhaps be objected that if sextant observations are liable to errors of this amount why has nothing been heard of it before? But you must remember that in all cases of the sextant being used the ship is at sea, out of sight of land, and ten miles one way or another makes no difference to her safety. The navigator has very little due to the existence of the errors of his observation. He compares them with his position by dead reckoning, and always assumes that the discrepancy lies at the door of the latter, and he works out the difference carefully, as "set and drift of current." If the navigator can rely on his observations in the ordinary way to two or three miles, that is about as much as he requires, and I think, with the sextants that are made nowadays, although they do read to 10″, that is about the accuracy one may expect.

I have gone into the question of the errors that are external to the sextant itself at some length, because it is clear that they are of great importance in the design of an instrument for sea work. If one minute (or possibly half a minute) is good enough for the reading, the sextant need not be of greater radius than four inches, instead of 6½″ or 7″, such as is generally found. The sextant can consequently be made very much lighter. The limb possibly could be cut or stamped from sheet metal instead of having a casting, and considerable economy in labour made.

Incidentally, if a casting must be used, why waste time and money in filing up the unimportant parts? A navigator does not want a sextant for its prettiness, and if he must pay the price makers now charge he would much prefer to have the money expended in valuable accessories rather than on useless finish or on improvements to the box.

I come now to the considerations of the errors inherent in the sextant itself, with a view to seeing how they should govern the design. The first of these is the error of centering: it is very difficult to cut the arc scale from exactly the same centre as that round which the index bar pivots. An error of one minute in the reading with a 4-inch sextant will be produced if there is an excentricity of 6/10,000 inch. The centre must therefore fit with that accuracy quite apart from the scale being engraved from the right centre. Such fitting as that with a taper bearing is easily obtained. A modern ball bearing will also satisfy the requirements. Beyond that point about the centre not being loose it does not matter how much out the centre is. The errors are always obtainable at Teddington, and it is no more trouble to the navigator to subtract 5 minutes from his angle than 40″. All that he would like to be sure about is that the centering error, as given in the N.P.L. certificate, is permanent.

That brings me to a very important point. When a sextant is being used with the sun, it is exposed to heat rays on one edge of the limb, and as the sextant maker finishes the limb, as dark as he can, that part absorbs heat and becomes distinctly hotter than the rest of the limb, and consequently expands. A displacement of the centre is inevitable, and an unknown imperceptible centering error is introduced whenever the navigator takes sights. The amount of this is only determinable by experi-

FIG. 3.

ments actually in the sun, in the Tropics, but I have reason to believe that some laboratory experiments I made, by warming up one part by current, was something approximating to service conditions. And I found a temporary centering error of 1′ 20″. The Kew "A" certificate, in which a maximum error of 40″ was allowable, is of not much value if temporary errors of that description arise in practice.

That temporary centering error would be almost entirely eliminated if the parts of the sextant that will be exposed to direct sunlight were polished or even painted a light grey. Any flat finish will do that will not absorb an undue amount of heat. The present style of finish is the worst that could be used.

Errors in cutting the arc and vernier are, I think, comparatively rare, though I have seen one or two cases. If the sextant limb is securely fastened to the table of the dividing engine, there is no risk of an error of this description.

Errors of the shades due to the shades being prismatic can be avoided if the prism is put with its base parallel to the face of the sextant. I am not advocating the use of prismatic shades, but they will do no harm at all as long as they can only be used in the manner I have stated.

Errors of want of parallelism in the mirrors produce duplication of images, and as long as no duplication is visible when a star is observed with the H.P. telescope with the index arm set at the highest angle possible, say 130°, no error in reading worth considering will result.

The errors of the second order, viz., perpendicular error, side error, and collimation error, will be found investigated fairly completely in a small book I wrote a few years back. The mathematical work in connection with them is long, and I will not weary you with it to-night. I must, however, mention that the three must be taken together: that the resulting error in the reading is not merely the sum of the errors due to each one separately, but includes three terms which are combinations of perpendicular and side error, side error and collimation, and collimation and perpendicular. These combined terms are approximately twice as large as the single ones.

Fortunately, the perpendicular error and side error can be corrected out fairly easily, but the arrangements for correcting collimation error are bad. However, as long as the side and perpendicular errors are corrected, the combined errors do not appear, and the resulting error in the reading is one due to collimation error only.

The actual value of this is $-\alpha^2 \tan \frac{R}{2}$, where $\alpha$ is the angle which the axis of the telescope makes with the plane of the arc, and R is the reading. This becomes at 120° approximately $\frac{\alpha^2}{2{,}000}$ where $\alpha$ is expressed in minutes. Half a minute of error in reading therefore corresponds to about half a degree of collimation error.

Now, practically the collimation error includes the angular distance from the centre of the field at which the observation is made. If this is half a degree, and the error of the centre of the field is half a degree in the same direction, the resulting error in the reading is two minutes. Of course it may be urged that the sight should be taken at the centre, but under adverse conditions, with a sight that has to be taken in a hurry, the navigator cannot always do this, and the sextant maker certainly ought to give the observer as much latitude as he possibly can; or, in other words, the collimation error of the centre of the field ought to be nil.

Collimation error is a difficult one to correct, and with the ordinary type of mounting to the telescope, the task is hopeless, for, owing to the bad design, a temporary collimation error is brought into play as soon as the end of the telescope is steadied at the navigator's eye. I have seen a sextant where as much as 1½ degrees of temporary collimation error, due to bending, have been found, so that with an extra half-degree from the centre of the field, the resulting error at 120° would be 8′. That sextant had an "A" certificate, and the errors in the reading given in the certificate were nowhere as much as 10″. It is quite conceivable that the purchaser paid an extra guinea, because it was supposed to be exceptionally good.

My own conviction is that if the telescope is suitably mounted there need never be any more collimation error than is due to the observation being made away from the centre of the field. This cannot be obtained if the telescope is mounted at one point only, but if it is mounted on the limb at two points the problem is easy. The few rough models I have here shew several ways in which this can be done. The mounting on the four-inch sextant is, I think, the simplest and best.

What conclusions can we draw from all our investigations? The most obvious one, I think, is that sextant makers have been aiming at refinements of scale accuracy without any regard to errors that come in from other sources, errors which are exceedingly large in comparison with the least reading of the instrument. To obtain this scale accuracy they have been forced to make the sextant far larger than is necessary, in order to be able to read these seconds of arc, and yet for sea work their instruments are literally very little better than Hadley's original quadrant.

The second conclusion is that it should be possible to make a small four-inch sextant for sea work only, in which the sextant errors can be trusted to lie within the least reading of the instrument, a reading that I should put at one minute. To obtain this freedom from error particular attention must be devoted to the exclusion of temporary decentration due to heating, and of temporary collimation error due to bending.

There are just one or two minor points of detail that I should like also to draw your attention to. The first is as to the relative position of the two mirrors. In so many sextants that I have seen the index shades cut off light by projecting into the wrong part of the beam. The accompanying diagram will shew this without any long explanation. I have seen a sextant in which this occurred to such an extent that when the index arm was set so as to measure the sun's diameter on and off the arc, as is done for the determination of index error, only *one* image of the sun could be seen. The other was completely cut off by the shade. That sextant had an "A" certificate.

**Eye-piece Shades.**—In the way these are fitted the pupil of the eye cannot be brought to the position of the Ramsden Circle, and there is a consequent cutting down of the field. It would be better to put a coloured shade over the O G. instead.

**Telescopes.**—Far too many telescopes and fittings are supplied. One is sufficient if it has a decently large O.G., so that with a low-power Huyghens eye-piece it will give a magnification of 4 for night work, and with the H.P. 6 to 8 diameters. The telescope should be mounted permanently on the sextant. The O.G. should preferably push in instead of screwing, so as to be easily removable. The eye-piece being also removable, the telescope tube can be used as a plain tube when no magnification is required. The ordinary star telescope is poor, and is not wanted, as the low-power eye-piece, with a large enough O.G., will do perfectly well for night work. I am strongly in favour of the telescope remaining on the sextant when it is in the case A sextant is often wanted in a hurry, and the time taken over fitting the telescope into the

Fig. 4.

collar may mean the loss of the sight. For the same reason I would have the focusing arrangement to consist of a simple draw motion up to a stop, that the owner of the sextant can set to his own focus and keep set there.

The cross wires in the telescope should be so arranged so as always to be parallel to the plane of the limb. Their distance apart should be about 1½ times the sun's diameter.

MICROMETER READING.—I would suggest the use of a micrometer for reading the fine divisions instead of the vernier. The latter has a way of getting slightly bent, and then becomes useless on account of parallax. The model I am shewing has been fitted with a micrometer, and as far as I can tell from the rough workmanship that I have been obliged to put into it, it would give very satisfactory results if better made. As it is, I can read this sextant with an accuracy of about half a minute.

SIGHT REPEATER.—I am strongly in favour of a prismatic attachment behind the horizon glass to enable the observer to get a set of three observations in quick succession without having to read his sextant afresh for each one. Such a fitting is shewn on this model.

On more accurate instruments a more elaborate system of prisms can be used; I have two other models, one for 5, and one for 9 observations at approximately equal intervals of altitude. I have also made one that made 27 sights possible without moving the index arm.

By means of this sight-repeating device a series of observations can be made without having to read the sextant for each one, and with a consequent avoidance of error due to hurried reading. The sextant reading, taken at the end of the set of observations, is the *mean* altitude for those sights.

# ON SOME NEW FORMS OF SPECTACLE LENSES.

BY DR. KLUGHARDT.

*Read 14th May, 1914.*

FROM the high position attained by the German opticians in so many branches of scientific and technical optics it was only natural that in its turn special attention should be directed to the optics of spectacle lenses, which had hitherto been sorely neglected. As we shall see, attempts in this direction have not been wanting, especially abroad, but it is only quite recently that the demand for more perfectly designed spectacle lenses has become acute, more especially since Gullstrand's work on the theory of spectacle lenses. I may therefore assume that you are interested in spectacle optics as a subject of special importance, to yourselves, and particularly in the Isokrystar and Punktal lenses, recently introduced by the firm of Emil Busch.

I cannot, of course, within the limits of a brief lecture, give you an exhaustive account, but must confine myself to the general outlines of the subject. I would refer those who may wish for more detailed information to the two excellent monographs by Dr. von Rohr, "Spectacles as an Optical Instrument" and "Spectacles and the Eye," the latter dealing with the problem in a very simple manner; [it is published in the series Aus Natur and Geisteswelt, of Messrs. Trubner]. Von Rohr has in these two books dealt with the whole of the problem of spectacle optics in his own concise and accurate manner. After this brief digression let us return to the subject itself, and briefly survey the practical development of these improvements

The problem opticians have been trying to solve since the beginning of the 19th century may be briefly stated as follows: To produce spectacle lenses which shall give *equally sharp images* of objects seen through the centre and the edge of the lens.

The earliest lenses known and made—the biconvex form—do not fulfil this condition at all.

Fig. 1.

The images formed by the outer parts are blurred and ill-defined, and a good image is obtained only when the line of sight is parallel to the axis of the lens (fig. 1).

The English physicist and physician Wollaston was the first to recognise this defect, and to take up the problem of correcting it. His very able discussion led to the choice of the meniscus form. His patent specification of February 9th, 1804—now over 100 years old—contains the following passage:—

"The object of my invention is to remedy the following defect which has been observed in spectacles hithertofore in use, namely, that no objects appear distinct through them, but such as are seen through the centres of the glasses or nearly so, but are indistinct in proportion to the distance of the part of the glass through which they are seen from the centre, whether sideways or upwards or downwards, that is to say the indistinctness is greater in proportion as the rays of light pass more obliquely through the glass; having observed that the portion of any glass employed in any one position of the eye is small, I perceived that by making the substance of the glass curved in the manner of a hollow globe, each portion of it might be situated nearly at right angles to the direction of sight, and would thereby render lateral objects distinct without impairing the distinctness at the centres." (Fig. 2)

FIG. 2.

—a statement which, by the way, is not quite correct, but which nevertheless led to a marked improvement as compared with the earlier lenses.

Spectacle lenses were actually produced in accordance with Wollaston's directions; but as these deeply curved glasses were very difficult and expensive to make their shape was gradually modified in various ways and less deeply curved forms were adopted. In this way periscopic, deep and shallow meniscus, globular or half globular lenses, etc., were evolved. All these lenses are so designed that one radius is kept constant for all or nearly all dioptric powers, while the other radius is chosen to give the desired power; but in this modification exact correction is not secured. While former opticians regarded the eye as a fixed system Wollaston's work shows that he had not neglected the rotation of the eye. According to von Rohr the first explicit reference to the movement of the eye and the centre of rotation is by Johannes Muller, the well-known physiologist, in 1826. There follows a long interval without any important scientific work on the subject.

FIG. 3.

The next work deserving attention is the dissertation of a young physician, August Muller, which appeared in 1889. Here we get an exact definition of the eye and the centre of rotation. He insists that in a periscopic lens, as he calls it, the refracting surfaces should always be perpendicular to the line of vision—a condition already laid down by Wollaston, and he determines the proper radius of curvature to be 30 mm. In his dissertation he says: "The centre of rotation of the eye lies 13.5 mm. behind the cornea. The posterior plane of a spectacle glass must necessarily be 15 mm. distant from the vertex of the cornea; the distance of the refracting surfaces from the centre of rotation is therefore 15 + 13.5 mm. = 28.5 mm. or approximately 30 mm.; and this number gives us the radius required. This shows that Muller had appreciated the importance of the centre of rotation, and had determined its position with some accuracy as 13.5 mm. behind the apex of the cornea. But he did not proceed to solve, either mathematically or by calculation, the problem which he had correctly stated; and in his discussion of the field of good definition he fell into error by relating it to the spherical aberration.

In 1898 we find another discussion of the problem by Ostwald, a French oculist. In a paper on periscopic glasses which has appeared in Graefe's "Archiven fur Optik," he states that two forms of spectacle lenses can be calculated one deeply and one less deeply curved, which fulfil the condition that good definition shall be obtained on looking through the edges of the lens. But the precise definition of the centre of rotation is not given in his exposition; and there are also errors in his formulæ, so that he did not completely solve the problem.

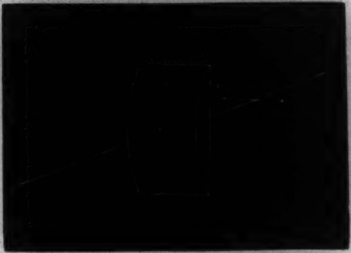

Fig. 4.

In England also attempts were made to solve this problem particularly by Percival, who followed the same lines as Ostwald.

The first to solve the problem correctly was Dr. Tscherning. In 1904 he published in the "Encyclopedie francaise d'ophthalmologie" an article on "Dioptrique oculaire" (the German publication is "Verres de lunettes orthoscopiques" in the August number, 1908, Archiv fur Optik, now unfortunately defunct). Using approximate formulæ he calculated spectacle lenses and pointed out the importance of the centre of rotation. From his calculations Tscherning found that for each dioptric system there are two different forms which have the advantage of giving sharp images for all directions. The improvement is obtained by correcting the astigmatism which we will consider more fully, later on. Tscherning, however, was not satisfied with this, and attempted at the same time to consider curvature of field and the correction of distortion. The complete removal of all three defects in a single lens is, however, impossible, and Tscherning therefore sought to determine those forms in which all these defects should be as small as possible. He named such lenses orthoscopic, and he had them manufactured and put on the market by the Paris firm of Benoit and Berthiot.

FIG 5.

Now, although the value of his efforts to improve spectacle lenses cannot be denied, it should, on the other hand, be pointed out that his attempt to take into account curvature of field and distortion was a mistake, as these defects are not nearly so prominent as the blurring at the edge of the field caused by astigmatism.

The next important suggestion for the further improvement of spectacle lenses is due to the Swedish oculist, Allvar Gullstrand, whose great services to science in this and allied directions have been recognised by the award of the Nobel Prize.

Gullstrand did not confine himself to the correct statement of the problem of calculating accurate spectacle lenses, but formulated anew a theory for the use of instruments in conjunction with the human eye and applied this theory very generally in its logical developments. It is this that has made his work so important for our subject, and on this account the leading optical instrument makers were induced to take up spectacle optics.

After this short historical survey, let us consider what happens when we look at an object. On close examination we recognise that the eye when looking at outside objects is in constant motion. It is really impossible without strain to keep the eye in the same position for any length of time. What then is the nature of this movement of the eye when it sees?

The muscles around the eyeball make it possible for the eye to make rotatory movements in all directions around one point, the centre of rotation. This movement, however, is limited in such a way that there never is any rotation of the eyeball around its axis in its normal position. In order to make this clearer, let us suppose that a cross has been marked on the surface of the cornea. We will notice that both lines of the cross remain approximately parallel for every position of the eye, when the eye is moving in its normal way. There is, therefore, no rotation of the cross round the centre.

This observation, which was first made by Listing, is of special importance to us, because otherwise it would be impossible to correct an astigmatic eye by any lens whatever for objects in directions other than the visual axis in the normal position.

The centre of rotation lies, as we have seen, inside the eyeball, and about 13 mm. behind the apex of the cornea. From the diagram you will see at once that all lines of vision must intersect in this centre of rotation. Thus these lines of vision represent for the spectacle lens, which is placed in front, the chief rays of our geometrical optics, and all these chief rays together form the pencil of chief rays. Von Rohr has called the hypothesis that all rays pass through the centre of rotation "Gullstrand's Condition."

FIG. 6.

When accommodation is relaxed (*i.e.*, when the eye is adapted for distant objects), the normal eye is focused on an infinitely remote plane, and the abnormal eye on the so-called far point surface, which is a sphere and is at a finite distance from the eye; the centre of this sphere coincides naturally with the centre of rotation of the eye. In the same way when the eye is focused on the near point this point will move over a sphere having its centre at the centre of rotation of the eye. Between these two surfaces lies the field of distinct vision within which every point can be distinctly seen by the eye. The mode of seeing thus defined with the eye in motion is called direct vision; by indirect vision we mean seeing with the eye in a fixed position as was assumed in the older ophthalmological optics.

We now come to the main point of the problem, which is, "In what manner must we correct a spectacle lens if it is to fulfil the condition already stated, that it shall produce sharply defined images when the lines of vision pass through its edges?" The answer is, briefly, "Above all, the astigmatism must be corrected." The reason why spectacle lenses give blurred images with an inclined line of vision

Fig. 7.

is the same as that found in photographic objectives for oblique beams—astigmatism.

What do we mean by astigmatism? You all know the cylindrical lenses used for the correction of the astigmatism of the eye. Their value depends on the fact that they produce an artificial astigmatism which is equal and opposite to that of the eye, and this neutralises its harmful effects. We will make this phenomenon clear by an example of a cylindrical lens, namely, a plano-cylinder. You are aware that a plano-cylinder refracts in one meridian only, and has no effect in the meridian at right angles. The diagram shows a perspective view of a cylindrical lens of which the axis is horizontal and a parallel beam of light falls normally on the lens. We consider two sections, namely, those given by two planes passing through the axis of beam. One of these planes contains the axis of the cylinder, and the other is at right angles to it. The first is horizontal, and the second vertical, and they are called the meridional and equatorial sections respectively (fig. 4).

If we now consider the rays in the vertical plane we notice that on passing into the glass they are refracted as if by a spherical surface, for in this vertical plane the surface of the cylinder acts as a spherical surface. We see how the rays of light of this plane unite in the focus of the refracting surface In the horizontal plane, however, the cylinder surface behaves as a plane-parallel plate, and the beams striking it parallel to the axis pass through without refraction.

FIG. 8.

Now we may draw other sections parallel to this vertical section, and the beams travelling in each of these planes will also focus in a point. These foci lie in one straight line which is called the focal line. The distance between this focal line and the points at which the beams of the horizontal plane intersect in infinity is called the astigmatic difference.

We will now take another example to explain the production of astigmatism by spherical surfaces. The next diagram illustrates another case. You will see there a small portion of the surface of a spherical lens on which a pencil of rays falls obliquely. As in the previous example, we can again choose for observation two principal sections perpendicular to one another which (as before) intersect in the axis of the pencil (fig. 5).

In the diagram you will see plainly how the pencil of rays which strikes the lens is refracted after its passage through the lens. The beams belonging to the vertical plane, *i.e.*, those of meridional section, intersect at point F1, but the rays in the horizontal plane, *i.e.*, in the equatorial section, intersect at point F2. There we also find at the focal points the focal lines which are perpendicular to one another and which can be seen distinctly in the diagram. The distance between the two focal lines measured along the axis of the pencil is also called the astigmatic difference. This phenomenon is called astigmatism for oblique pencils.

FIG. 9.

The length between the two focal lines, *i.e.*, the astigmatic difference, therefore depends not only on the curvature of the planes, but also on the angle of incidence. On the axis there is no astigmatism; the image is really sharp. When we trace the foci of the two focal points for different angles of inclination we get two curves (the image curves, fig. 6). Through rotation of these curves about the axis we get the two image surfaces—the meridional and the tangential. The outcome of these phenomena is a peculiar blurring of the image.

If we look at a trellis-work fence through a cylindrical lens we can find a focus at which the horizontal lines are sharply defined, and the vertical ones appear quite blurred. The very opposite takes place with a different focusing, and you cannot get the vertical and the horizontal lines sharp at the same time. This is clearly shown in the diagram, fig. 7. Thus astigmatism can be caused at any spherical surface.

Let us turn from these theoretical considerations to the practical subject of spectacle lenses, and see what is the effect of astigmatism in these lenses and how it can be corrected. We have already shown that astigmatism may appear at any refracting surface, and that it does occur as soon as a pencil of rays falls obliquely on the first surface of a spectacle lens, which in this instance we will suppose to be convex. This pencil, which is already astigmatically affected, travels on and strikes the second surface of the spectacle lens. What happens? There are three possibilities, according to the curvature of the surface. If the second plane be also convex, *i.e.*, converging as in a bi-convex lens, the astigmatism of the second surface will naturally be added to that of the first, and we get an increase of astigmatism. If, however, the second surface be diverging, *i.e.*, *concave*, an opposite astigmatism is produced at this surface, which

BICONCAVE. FIG. 10. ISOKRYSTAR.

tends to compensate the astigmatism produced at the first surface, and we are led to suspect that there is a relation between the two curvatures for which the astigmatic defects of the two separate surfaces neutralise one another, so that we could obtain a spectacle lens free from astigmatism.

On calculating we actually find that, within certain limits, it is possible to obtain such lenses both for the plus and the minus powers; indeed, there are—as Tscherning discovered—always two forms for each power—a deep and a shallow meniscus lens.

The former has been called Wollaston's form after the English physician already mentioned, the latter Ostwald's form, after the French oculist. The diagram shows the two forms for – 5.0 and + 5.0 D. (Figs. 8 and 9.)

FIG. 11. POWER – 20 D.

The correction obtained from the two forms is practically the same; but as the deep meniscus lens increases the cost of production considerably, and at the same time (on account of its deeply curved surfaces) does not look as well as the shallow form, we have taken the last named form—Ostwald's—as the basis of the Isokrystar spectacle lenses.

In the mathematical calculation of the Isokrystar lenses the point of intersection of the chief rays, or its distance from the spectacle lenses, plays a considerable part, in so far as the astigmatism depends likewise upon this quantity. This distance, however, cannot be used for the removal of astigmatic defect, as this is a fixed quantity from the outset because, as we have already seen, this distance is just the distance of the centre of rotation from the spectacle lens.

In this respect the Isokrystar lenses are carefully corrected for all oblique pencils of rays within angles of 30° on each side of the axis, and therefore, when the lens is in the prescribed position the eye is bound to see in every direction an image of equal sharpness to that seen through the centre of the lens.

Now it seemed obvious to call such spectacle lenses "anastigmatic spectacle lenses," by analogy to photographic lenses corrected for astigmatism. But as astigmatic, *i.e.*, cylindrical lenses, are already known in visual optics, it is more advisable to reserve the expression "astigmatic spectacle lenses" for what are called spherical spectacle lenses which show no astigmatism on the axis, and accordingly Gullstrand has suggested that these new spectacle lenses should be called "point image" lenses, because they permit every point of the outside world for the entire field of vision to be represented by a point on the retina.

Now, the Isokrystar lenses belong to this class of "point" image lenses, and I should like, in confirmation of my previous remarks, to demonstrate clearly by means of diagrams in what respect these lenses are superior to those hitherto in use for spectacles.

In the first place let me show you some curves as found by trigonometrical calculation.

In the next diagram you will see the curves for astigmatism in a bi-concave, and in an Isokrystar lens for −10.00 D. The next diagram shows the curves for these lenses for −20.00 D, and the third one for +5.0 D (biconvex Isokrystar). (Figs. 10, 11, 12.)

From these curves you will plainly see that in ordinary bi-lenses the astigmatic difference increases with increasing power and inclination of the chief rays, and that the Isokrystar lens is practically free from astigmatism, even for such large angles of inclination as 30°.

But in order to give you a practical proof of the superiority of point image spectacle lenses, I will show you a number of photographs. These have been taken in the following way: In front of the objective of a photographic camera was placed a bi-convex lens of −10.00 D (well centered), and at a distance of 26 mm. from it, between the objective and the spectacle lens, was put a stop of 5 mm. diameter, *i.e.*, equal to the average diameter of the pupil of the eye. (See fig. 13.)

Fig. 12. Biconvex, Periscopic and Isokrystar, showing relative Astigmatism.

At some distance from this arrangement were placed the test types. The camera was then precisely focused through the spectacle lens on the letters opposite the axis, and a picture was taken. Then the spectacle lens and the stops were turned in such a way that their axis made an angle of 10° with the axis of the objective, and in this position another picture was taken without any alteration having been made in the focusing of the camera, and so on until an angle of 30° was reached. It was found that with a bi-lens an inclination of the axis of 15°—20° was sufficient to produce a slightly blurred picture; at an angle of 30° the letters were quite blurred. On making the same experiment with an Isokrystar lens it appears that the letters appear clear and distinct at an angle of 30°. These experiments show diagrammatically what takes place in direct vision when the eye moves.

In the same way various pictures have been taken which show a similar result with bi-concave lenses and Isokrystar lenses, *e.g.*, with – 10.0 D. Moreover, I will let you see these phenomena for yourselves at the end of the lecture by the aid of a special piece of apparatus.

FIG. 13.

I have thus placed before you the principle of correction and the behaviour of point image spectacle lenses, and you will all have been convinced how insufficient and ineffective the old spectacle lenses are and how clear and distinct the fields of vision become by means of the new "point image" spectacle lenses.

So far I have only spoken of the "point image" lenses with purely spherical power, *i.e.*, such as show no astigmatism on the optical axis, and thus are called anastigmatic lenses. I will now say a few words about astigmatic lenses, *i.e.*, lenses with cylindrical or sphero-cylindrical power, and about the way in which a point image result can be obtained from these.

In purely spherical lenses a knowledge of the rotation of the eye and the centre of rotation is sufficient for the correction of the astigmatism at the edges of spectacle lenses, and it is not necessary to consider the details of this rotation. But with astigmatic (cylindrical) lenses a more exact appreciation of the rotation of the eye is absolutely necessary. According to Listing's law, already referred to, it is possible to correct the astigmatism of the eye, as its principal meridians do not rotate about the visual axis when the eye is directed to different objects. Thus the astigmatic spectacle lens can be arranged with its principal meridian in such a way that it corrects the astigmatism of the eye in any position; and this is the case when the principal meridians of the eye concide with those of the spectacle lens.

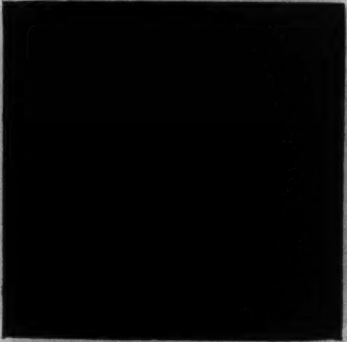

Fig. 14.

You will remember the diagrams which showed the correction of astigmatic aberrations of the spherical lenses, and you know how the astigmatism of oblique pencils of rays is fully corrected for all angles of vision in these lenses. Of course, this conception does not apply to the astigmatic point image lenses (having cylindrical power on the axis), as the special feature of this case is that the correcting lens should possess a certain amount of astigmatism so as to correct the error in the eye of the ametrope. But there can be no doubt that, as a blurring of the image occurs in ordinary purely spherical lenses with oblique vision, the amount of astigmatism present on the axis of cylindrical and sphero-cylindrical lenses will also vary as the line of sight moves from the axis to the edge, and these changes will generally be more important than with purely cylindrical lenses. Thus it might happen that a lens which has a cylindrical power of 1 D on the axis might have several times that amount at the edge.

Again, with lenses of purely cylindrical power the astigmatic correction changes quite considerably when the eye looks obliquely through the lens, and this change varies according as the movement of the eye is in the direction of the axis of the cylinder (fig. 14, faintly marked line), or perpendicular to it (fig. 14, strongly marked line). The case is still more complicated when the angle of vision moves obliquely between the two axes (fig. 14, dotted line). It is possible to correct these changes of astigmatic effect, or at least to reduce them within permissible limits, by a suitable form of the lens with the correct toric surfaces.

The point image lenses of the firm of Busch are made so that the convex front is ground with a toric surface, and the concave back with a spherical surface.

We will again illustrate, by means of photographs, the effect of the point image lenses as compared with ordinary sphero-cylindrical lenses. The next diagram shows photos taken first with a sphero-cylindrical lens, and then with a point image lens of the same power. Lines 1, 2 and 3 are photos of a specimen of writing taken through sphero-cylindrical lenses, and lines 4, 5 and 6 show the same writing taken through point image lenses of equal power. In lines 1 and 4 the rotation of the eye is in the plane of the axis of the cylinder, in lines 2 and 5 at right angles to the axis, and in lines 3 and 6 the rotation is in a plane inclined at 45° to the axis. This diagram once more demonstrates the superiority of the point image astigmatic spectacle lenses as compared with the simple sphero-cylindrical lenses hitherto in general use (fig. 18).

Fig. 15.

There is one matter which I should like to point out, and which is of some importance with the Isokrystar and Punktal lenses. You will have noticed on the diagram that all chief rays undergo a change of direction when passing through a spectacle lens with the exception of the ray which passes through the optical centre. It follows that the lines of sight of any eye using a spectacle lens are not the same in front of and behind the lens, therefore, such an eye sees an object in a somewhat different direction from an unaided eye.

This is harmless in binocular vision if the lenses have been well centered and adjusted and the powers are not markedly different; for in that case the change of direction of lines of sight is the same for both eyes, and the corresponding chief rays run parallel in front of and behind the spectacle lens. In this case the wearer of the glasses does not notice the change of direction at all.

But it is a very different matter when the lenses have been badly adjusted. In that case the change of direction through the spectacle lenses may have an injurious effect on the eyes.

If the spectacle lens has not been accurately centered the eye in its normal position, looking straight ahead, sees objects in an oblique direction, and the lens has the effect of a prism. In the slide a convex lens is taken, and you can see plainly the path of rays with a wrongly centered lens.

FIG. 16.

When the eye is in its normal position, and is really looking straight ahead, this line of sight suffers a change of direction sideways through a badly centered lens. With a convex lens of 5.0 D a decentration of 1 mm., the angle amounts to 18′, *i.e.*, the decentration of 1 mm. has the effect of an apparent displacement of the object of 5 cm. at a distance of 10 m. With a decentration of 2 mm. the angle amounts to 34.2′, and the apparent movement of the object becomes 10 cm. distance; with 3 mm. decentration 51.6′, or 15 cm. at 10 m.

This phenomenon is apt to become very unpleasant in binocular vision. Suppose we assume (as in the diagram) the distance of the centre of the spectacle lenses to be greater than the interpupillary distance, we get opposite deviations from the normal line of vision for both eyes—a condition which is bound to be very troublesome because the eyes must squint continually in order to compensate this deviation of the object. The distance of the spectacle lens from the cornea is also of importance. In calculating the Isokrystar and Punktal lenses the distance between the centre of rotation and the surfaces of the lens was taken as 26 mm. As the distance between the apex of the cornea and the centre of rotation amounts to 13 mm., according to Gullstrand, it follows that the distance between the apex of the cornea and the posterior surface of the lens is 13 mm., a distance which has proved convenient in practice.

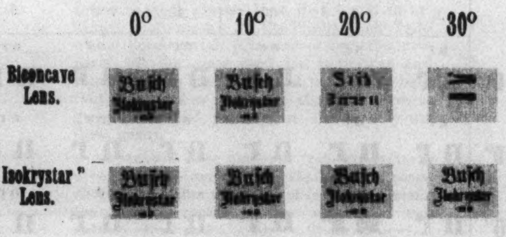

Fig. 17.

From this it is clear that it takes a practised optician to adjust these glasses properly, as the best results can only be obtained when the lenses are accurately adjusted.

In considering the practical manufacture of such lenses the choice of glass is the first important point. The special glass in use for ordinary spectacles is fairly colourless and free from bubbles, but it is by no means equal to the modern Jena boro-silicate glasses in permanence and freedom from colour. The optical glass has the further advantage that its optical properties, especially refractive index, can be maintained in new meltings. For this reason the firm of Busch have selected a perfectly white extra hard boro-silicate crown glass, such as is used for the prisms of field glasses.

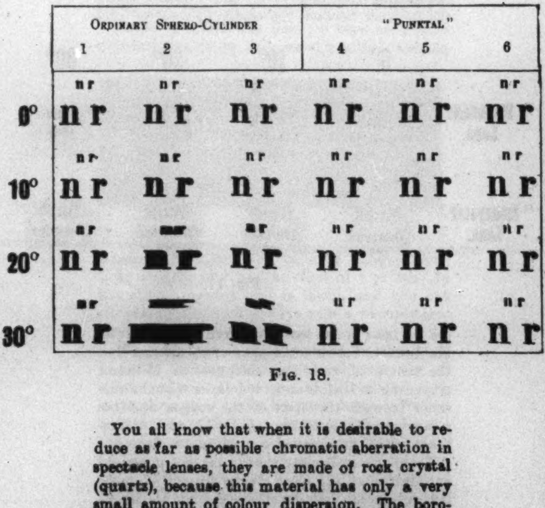

FIG. 18.

You all know that when it is desirable to reduce as far as possible chromatic aberration in spectacle lenses, they are made of rock crystal (quartz), because this material has only a very small amount of colour dispersion. The boro-silicate crown glass of the Isokrystar and Punktal lenses stands half-way between the glass now in ordinary use and the quartz, as regards its colour dispersion. It will be seen from this that these lenses are superior to ordinary spectacle lenses on account of their slighter colour defects. The special hardness of this glass, which can be noticed in edging, is a protection against injury to the polished faces from cuts, scratches or blows. Because of its greater hardness the glass is more difficult to melt and not so easily liquified as other crown glasses. On this account tiny bubbles cannot be avoided during the melting, and these are kept out by careful selection of the material to be used for Isokrystar and Punktal lenses.

Also the resistance of this glass to atmospheric action is much greater than that found in ordinary plate glass. At the Physikalisch Technische Reichsnastalt (Charlottenburg) this property of the glass used for Isokrystar and Punktal lenses has been tested, and it has been found to have a resistance about 12 times as great as that of plate glass—if one may use these terms.

As you will have gathered from my remarks a considerable amount of calculation is necessary to determine the forms of Isokrystar lenses, the proper curves being separately calculated for each power. Their manufacture is more difficult on account of the number of different tools required and the deepness of their curves, and the greater accuracy that is necessary. The testing is stricter and there is more waste owing to the small bubbles already mentioned. In consideration of all these facts it is not surprising that the price of the lenses is necessarily higher.

Seeing, however, the extraordinary advance in the effectiveness of Isokrystar and Punktal lenses, it is not anticipated that the somewhat higher price will restrict the demand, particularly if the optician is capable of explaining the obvious advantages of such lenses.

## Discussion.

**Mr. S. D. Chalmers** said that he desired to explain that the manuscript of Dr. Klughardt's paper had been sent in German, and a translation had been made. He hoped the translator had been successful in retaining the general meaning of the original, though it was always difficult to keep the exact shade of meaning. The term meniscus was used in the paper, as applying to any lens of which one surface was concave and the other convex, and not to one particular form. The term "axis of the lens" was used in the ordinary way, just as for the axis of a spectacle lens. The term "axis of the cylinder" would, of course, have its ordinary meaning.

**The Chairman** (Dr. Wm. Ettles, President of the Society) expressed the thanks of the meeting to Mr. Chalmers for the able translation and the lucid way in which he had brought forward Dr. Klughardt's paper, which was interesting in many ways. He hoped they were going to get a useful discussion on many of the points raised. There were several visitors present, and he wanted them all to consider themselves members of the Society for the evening, and that they would join in a free and unfettered discussion of the paper. In order to start the discussion, he might inflict upon them one or two comments which had occurred to him. He was by no means certain that Müller had been the first to think about the centre of the rotation of the eye. Porterfield, of Edinburgh, had written a very learned book about 1759 on "The Manner and Phenomena of Vision." In that he very accurately described the theory of the centre of rotation. Another point which occurred to him was that it was all very well to compare the best possible ellipsoidal lenses with the worst possible spherical lens, but he would like to see a comparison made between the best possible toric lens and this new lens, in which case the difference would be much less marked. It had yet to be proved that the centre of rotation of the eye was a perfectly fixed point. The general feeling at the present moment was that there was a different centre of rotation for each pair of muscles. That made talking about a centre of rotation rather difficult—at any rate it knocked out the half millimeter. Another point which occurred to him was that eyes do not rotate about behind spectacle lenses to the degree postulated; one did not look 60 degrees across one's field without moving the head. Physiologically what happened was this. Our eyes, it was true, were in a state of continuous movement. But that continuous movement was about a very small excursion. They did not move more than 10 or 15 degrees either side of the direct axis. When one turned one's head and an image fell upon the periphery of the retina, which although it was very insensitive as regards form vision was acutely sensitive to any kind of movement—more sensitive even than the macula—one at once turned one's head until one's critical vision came within that little projected cone in space whose angle would be about 15 or 20 degrees. Therefore for the ordinary purposes of life it was found that the definition afforded by the ordinary bi-concave or bi-convex lens was sufficient. One did not find people complaining of low-power bi-concave or bi-convex lenses. It was only with high-power lenses that one got that exaggerated head turning which characterises very ametropic persons. Another very important point from the selling point of view and from the oculist's point of view was the appearance. The curves might be very beautifully plotted out, and susceptible of scientific proof, but it was impossible to convince a young lady who had to wear glasses that things which looked like policemen's bull's-eyes were as nice as ordinary glasses. That was a real difficulty in practical application, and he (the speaker) wanted to know what was the lowest curve that could be got consistent with getting perfect definition within the small angle which he considered necessary. Lastly, he was under the impression that it had long been known that borosilicate crown glass was very light, very hard, and very suitable for spectacle making purposes, and he would like to know whether manufacturers of glass in England were not able to fulfil some of the requirements so ably indicated by the writer of the paper.

**Mr. Mackinney** said that undoubtedly borosilicate glass almost identical in character with that described in the paper could be obtained in England.

**Mr. W. A. Dixey** said there were one or two matters which as a maker of spectacles he might be allowed to mention. He sympathised with what the President had said about low power biconvex and bi-concave lenses. On the other hand he, and to his knowledge many other spectacle makers in England, had long used Dr. Percival's scale of values for periscopic lenses. Originally they had used Ostwald's scale, but when Dr. Percival read his paper about 12 or 14 years ago they had substituted Dr. Percival's because his formula seemed more accurate. He (the speaker) was open to conviction, but he could not see any difference in optical principle between these Isokrystar glasses and the periscopic glasses that were advocated by Ostwald and Percival. Tscherning had tried to introduce a correction for barrel distortion, which made a little difference to the form of the glass, but he did not think it had made any real practical difference to the effect of the lens to the eye, and he ventured to suggest that the English periscopic glasses advocated by Dr. Percival still held the field for cases for which such glasses were suited. It seemed to him, though here he spoke with some little diffidence, that Dr. Klughardt was perhaps better acquainted with optical problems affecting instruments than with optical problems affecting spectacle makers. He had read Dr. von. Rohr's book, which he understood was now in course of translation, and with regard to that he had the same criticism to make. It was beautiful reading, but hardly practical. It dealt with the optical problems which confronted the telescope maker and the photographic lens maker, and had no real application to spectacle makers at all, because after all the only aberration that was at all to be considered in spectacle lens making was radial astigmatism, which was being adequately corrected, and had been for the last 30 or 40 years, by the periscopic form of glass Dr. Klughardt did not appear to distinguish between zonal and radial astigmatism. He was so imbued with the importance of correcting zonal astigmatism in a large aperture photographic lens that apparently he had allowed it to influence his consideration of spectacle lenses in which zonal astigmatism did not count at all, because of the smallness of the aperture of the eye. He was a little disappointed that Dr. Klughardt, in sending his paper to the Optical Society of London, had not favoured them with the mathematical calculation from which his conclusions were drawn. He had tried to follow the way in which the photographic diagram which had been shewn upon the screen had been taken in the camera, and he thought he was right in saying that the errors had been very much exaggerated. It was obvious that the loss of definition was in direct proportion to the distance of the lens from the image, and he strongly suspected that the errors which they were going to see through the instrument which was lying on the table were exaggerated in the same way, namely, by lengthening the distance between the lens forming the image and the screen which received it. He thought it was useless going into very minute calculations in these matters. They had to be satisfied with approximations. What was the use of entering into minute calculations and correcting the curves down to a millimeter of radius, if they were not going to take as a factor in the calculation the different physical dimensions of each individual eye to which the lens was going to be applied? These physical dimensions constituted some of the most important factors in these calculations, and as the dimensions could, as a matter of fact, only be averaged, it seemed useless taking average values and regarding them as constants, whereas they were not constants, but variables, varying with every individual to whom the lens was applied. They must rest satisfied, as he had said, with approximations, and by doing that they had done, and would continue to do, excellent work.

**Dr. Drysdale** said he had a suspicion that in these very deep lenses, when it came to peripheral vision, the question of the colour produced was rather serious—sufficient, in fact, to mask the improvement produced by the more accurate curves. He would like to point out that the scale of the instrument was coloured red, presumably for the purpose of eliminating the effect of chromatic aberration. Some of the defects concerning the astigmatic effects of lenses could be rather easily tackled by the system of optics known as the curvature system. Various phenomena could be shewn much more readily by that system, and perhaps this was not as generally known as it might be in the optical trade.

**Mr. Chalmers** said the problem before the mind of the person who designed the lens in question was not to produce perfect definition. The problem was to produce the same definition with the same eye, whether looking through the centre of, or obliquely through, the lens. The other problem was a problem for the oculist or optician. If the oculist would prescribe something which would give perfect definition, then the designer would set out to design a lens to meet the case. The method of calculation he believed was the strictly accurate method of trigonometrical calculation, and the lenses would strictly fulfil the conditions, which the designer set out to fulfil, of freedom from radial astigmatism. If he might venture upon a criticism, it appeared to him that Dr. Klughardt had not stated his problem quite correctly, because he had set out to make a lens which would give a perfect definition at infinity. Some time ago he (the speaker) had occasion to work out a certain periscopic correction for near vision, and the result was that if the object were taken at 33 cm. from the eye it was necessary to modify the form quite considerably before equally good definition could be obtained. In the periscopic form the difference on the negative surface was as much as 3D when designed for a near object as compared with one designed for infinity. His own view of the proper way to state the problem was this. One should design the lens so that the error given for a distant object should be as great as was permissible. He thought the President would agree with him that it was most undesirable to overcorrect errors in a case of this kind, and that was what happened when a lens was designed for infinity and then used for near vision. He thought it was a good thing for the industry that the optical instrument firms had taken up the problem of making spectacle lenses accurately. Some years ago he had had occasion to work out the best forms of toric lenses, and he had come to the conclusion that with the toric surface in use at that time, the imperfections of the curves were often more than enough to destroy the improvement one might obtain in using the proper form. He thought it would be a very great service to optics in England if those who had knowledge of the actual periscopic forms of lenses used before the days of Gullstrand were to put these on record.

### The Demonstration Instrument.

Mr. Purser said the instrument shewn contained two lenses, one an ordinary bi-concave lens of about 14D, and another the new form of lens. The colour of the diagram and the small ruby lens was in order to avoid the effect of colour dispersion, not only on account of the new lens, but also on account of the ordinary lens, no claim being made by the designers of the lenses to correct chromatic aberration.

**The Chairman** said he was rather amused at the suggestion that the oculist should provide the perfect correction, because nowadays, owing to the vastly increased skill and capacity of the sight-testing optician, the oculist only got cases which were referred to him by the optician on the ground that the vision remained subnormal in spite of the correction of the refractive error.

**Mr. Chalmers** thought the paper constituted an exceedingly fair statement of the case as to what was actually done with that particular lens. One must realise that the writer lived in a country where the lens of the Ostwald and Tscherning type had not had very considerable use. They had had in England, and still more in America, a great deal of experience in periscopic lenses in actual use, but in Germany—and in France also, he believed—their use had been exceedingly limited. One must allow for the fact that the writer would naturally tend to compare the lenses with the best in use in his own country.

**Mr. Dixey** asked whether Dr. Klughardt had published his calculations.

### Methods of Calculation.

**Mr. Chalmers** replied in the negative. Dr Klughardt had made the ordinary trigonometrical calculations, which were generally used in calculating photographic lenses—the actual words used in the German showed that this was the case, and he thought it might be taken that the actual solution he had obtained was the correct result of the problem he had set himself. Perhaps Dr. Klughardt had not done full justice to the great advances made by Mr. Percival and Dr. Ostwald, but it must in fairness be realised that in Germany these advances had not been practically applied, and Dr. Klughardt was really quoting from his own experience, where the new lenses were compared, not with reasonably good periscopic lenses with standard base curves, or the Percival forms, but with bi-convex and bi-concave lenses. He had also stated that the periscopic lenses in common use were not the best possible theoretically—in that he was quite right in his (Mr. Chalmers') opinion. The curves used in the optical workshop were almost always given with the standard base curve. Most wholesale houses, he believed, when dealing with periscopic lenses, took a certain base curve, and calculated the power from that, and he thought he was right in saying that it was not the general practice to use more than two base curves.

Mr. Dixey said that was true of toric, but not of periscopic lenses.

Mr. Purser said that the base curves in common use, and produced by 90 per cent. of the workshops of the world, were as a rule for those known as ordinary periscopic lenses, 1.50 D and 3 D, for the deeper lenses 6 D and 9 D. They were the regular base curves on which probably every wholesale manufacturer in London and Paris worked at the present time, and he would guarantee to say that a very small percentage indeed of lenses were made in any other way. Most of the criticism seemed to him to be aimed not at the scientific construction of the lenses, but at one or two minor points which it appeared to him were not of very much consequence. Two or three speakers had commented on the fact of the boro-silicate glass being known and used in England, but he did not see that that had anything to do with the lecture itself. The writer only stated that the glass used was boro-silicate, and had certain properties, but he thought it could safely be said that in the manufacture of spectacle lenses by any British or French or American house boro-silicate glass was not in use. That was quite a safe statement, and therefore in using the new glass for these new lenses quite a new departure had been made—and a departure he was quite sure any who had handled it would be convinced was a great advance.

**The Chairman** said that the criticisms he had made on the paper must not be taken to imply that they were not all very grateful to Dr. Klughardt for sending them his paper.

**Mr. Chalmers** pointed out that the claim made on behalf of the boro-silicate glass was that what was known as good optical glass was being used instead of ordinary spectacle glass. There were differences in the quality, and in some respects it was much better to employ optical glass rather than the ordinary spectacle lens glass.

**The Chairman** asked whether that was a new discovery.

**Mr. Chalmers** replied that it was not a new discovery, but it was an attempt to take advantage of the better optical knowledge and the better materials which were now available, and which were not being so fully used as they might be by the spectable industry generally. It was fair to say that in this respect the advantage was probably greater in Germany than in England, because the ordinary spectacle glass in Germany was not well suited to spectacle lenses. It was soft and not very satisfactory. The French was harder and somewhat better glass; but the glass made in England had already approached towards the type of boro-silicate crown glass. Still it had not been made to the highest degree of perfection. The difference in cost was a difference as between 5s. per pound and 1s. per pound, and they would all realise that that meant a considerable difference in quality. The makers of that particular lens claimed to be using the best material possible, the hardest glass possible, and therefore it possessed advantages over the ordinary lens. He thought the makers of these lenses were quite right to call the attention of the industry in general to the fact that advantages were to be gained by using better glass. From the optical, as distinguished from the manufacturing point of view, he (Mr. Chalmers) thought they were right.

A **Member** asked whether it would not be possible for them to have more definite information about the actual differences in the curves as between the periscopic and the Isokrystar lenses. He would be glad if anyone could give him information as to whether in the higher degrees the Isokrystar lenses had been actually found by the wearers to give an advantage.

**Mr. Mackinney** said that the boro-silicate glass undoubtedly gave definite advantages for spectacle work. There was every reason why the best price possible should be paid for glass used in spectacle work.

**Mr. Dixey** asked what was the difference in principle between these lenses, as described in the paper and lenses worked to Dr. Percival's formula.

**Mr. Chalmers** replied that he had not examined in detail the actual curves of the lens. He did not know how closely the curves given in the patent specification for the astigmatic lenses agreed with Dr. Percival's, but he was under the impression that some of the curves were like those he had seen before, but whether any of Dr. Percival's curves came within the limits indicated in the specification he did not know.

**Mr. Green** said that Dr. Klughardt in his paper had said that Tscherning sought to determine those forms of lenses in which astigmatism, curvature of field and distortion, should be as small as possible, but Tscherning, in his paper,* gives two sets of curves called A and B for an object at infinity, and two sets for an object distance of 33 cms. for no astigmatism, and two sets of curves for no distortion for a distant object both in the plus and minus powers. And writing of such lenses Tscherning says: "There is no lens which fulfils the three conditions at once, but we find that the anastigmatic form B is a near approach to the orthoscopic A. For that reason the anastigmatic B lenses produce but little distortion. The firm of Benoit and Berthiot has willingly made a set of lenses of this form under my directions; commercially they are known as orthoscopic lenses." He had some of the curves with him and, with the permission of the meeting, he would put them on the blackboard. The standard periscopic lens used in fully 90 per cent. of the cases had a base curve of ±1.25 D. The standard meniscus lens used in fully 90 per cent. of the cases had a base curve of ±6 D. Sometimes base curves of ±3 and ±9 were used, but the 9 D, on account of the peculiar appearance it gave to the patient, was rarely requested. The French had no standard base curve; the base curve, especially in the high powers, was reduced in order to reduce the power of the opposite surface; this for economical reasons.

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*Recueil d'Ophtalmologie, Janvier, 1909.

The whole trouble with these modern lenses was the fact that not only were the curves exceedingly steep, difficult and costly to produce, but they varied for every power. In workshop practice they wanted to have, if possible, a standard curve for plus and for minus, and some of the powers of the standard types of lenses at present in use were quite accurate for all purposes. It was stated in the paper that there were alternative curves of a low order, and it was that low order that one would like to have, both for cosmetic and other reasons. Curves of very steep character, such as those of Tscherning's, were very difficult and costly to produce, and, following Dr. Klughardt's line of argument, he would prefer the Percival or

| TYPE. | DESIGNER. | POWER. | CURVES. | ANGLE. |
|---|---|---|---|---|
| American Standard | — | +2 D | + 8.0 − 6.0 | — |
| Orthoscopic | Tscherning | +2 D | +19.5 − 17.5 | — |
| Wollaston | Whitwell | +2 D | +18.9 − 16.9 | 60° |
| Ostwalt | Whitwell | +2 D | + 9.5 − 7.5 | 60° |
| Isokrystar | Klughardt | +2 D | +10.5 − 8.5 | 60° |
| — | Percival | +2 D | + 8.0 − 6.0 | 50° |
| American Standard | — | −4 D | + 6.0 − 10.0 | — |
| Orthoscopic | Tscherning | −4 D | +18.0 − 22.0 | — |
| Wollaston | Whitwell | −4 D | +17.2 − 21.2 | 60° |
| Ostwalt | Whitwell | −4 D | +5.26 − 9.26 | 60° |
| Isokrystar | Klughardt | −4 D | + 6.0 − 10.0 | 60° |
| — | Percival | −4 D | + 4.0 − 8.0 | 50° |

the standard American meniscus lens, or the Ostwalt type, to Tscherning's or the Isokrystar, because the curves were not so steep. He was quite sure that if some of the mathematicians who were good enough to calculate these curves had to wear the glasses themselves, and were mindful of their own appearance, they would much prefer to adopt the alternative and wear glasses which were not so mathematically correct. Another argument in favour of the standard base curve was that it facilitated the handling of large quantities of lenses from the manufacturer's point of view. In the case of bifocal lenses, for instance, if the inner or the outer curve were not a standard such a thing as bifocal work would become impracticable from an economic point of view. With regard to the question of glass, he had carefully examined the glass from which Isokrystar lenses were made, and he had not found it quite so free from colour as the hard, colourless crown glass in present -use for the making of spectacle lenses. He would be inclined to contest the statement in the paper that the surface of the glass was more permanent than that at present in use. He had never seen a piece of glass used by American or French manufacturers, nor had he seen a single lens in the whole of his experience which had undergone surface deterioration. If by that is meant the alteration of the surface polish, such as occurred in flint glass. The question at issue was not boro-silicate crown versus plate glass but boro-silicate crown versus hard crown. He certainly thought that Dr. Klughardt should have chosen the best lenses that were at present in use and compared them with the lenses for which he was responsible. He believed that Dr. Klughardt would then have had a very different story to have told us.

Mr. Baker said he could not speak with any authority on the subject of spectacle work, but he understood that the general trend of Dr. Klughardt's paper was that this special form of lens had been manufactured for the purpose of correcting astigmatism for a definite position at infinity, and a definite position of stop, for the centre of rotation of the eye. He gathered that the position of centre of rotation of the eye was not by any means a fixed point, and further that the vision of the ordinary human being was required for other distances than infinity. His own experience of the possibility of working out ordinary lenses for the purpose of correcting astigmatism was that the position of the image and the position of the object were absolutely vital for the purpose of calculating the two curves of the lens, and that in certain positions of the stop a very small movement of the stop one way or the other would affect those curves considerably. If the centre of rotation of the eye was not a fixed point, and one were to allow for movement one way or the other, then he was perfectly certain that the stop position was in such a position relatively to the lens that very different curves would be required. If they took people with a centre not always in the same place, a small movement of the stop position would affect the value of the curves quite distinctly. He thought that to talk about making curves correct to $\frac{1}{2}$ D was a refinement of accuracy which was absolutely impracticable.

**The Chairman** said he could not believe that the centre of rotation was the same for different errors. He had eyes in his collection whose axial length was very considerable, and no doubt some of them had seen them at the Northampton Institute. No doubt there were gentlemen present in that room who had seen myopic eyes whose curvature would be as illustrated at p. 137, and that was by no means an exaggeration. In the case of an eye with, say, 20 D of myopia (which was not very uncommon), since 3 D = 1 mm., the increased axial lengthening would be 7 mm., and, taking a normal eye as being 24, practically it would be increased one-third. They were bound to shift that centre of rotation, or else the statement that myopia was axial at all could not be true. So that there was an assumption that had been left out of account. It had been assumed that for all lenses the centre of rotation did not vary for all conditions of refraction. It appealed to his common sense that it must vary enormously, and he was persuaded that it did.

## Ocular Centre of Rotation.

Mr. Purser submitted that that statement was not made, and it did not follow at all. It was definitely stated in the paper that an approximation—a medium average distance—was taken. It was quite clear that no man, knowing that the distance or the size of each eye varied, would attempt to calculate on the basis that they were all fixed measurements. That was impossible. In the paper he thought there was a definite statement that an average size was taken for the calculation of this measurement.

Mr. Dixey thought he had raised the point himself—at all events, he had meant to do so—that if they were going in for this minute calculation, they must really make a different calculation for each eye they were going to correct.

Mr. Chalmers said that the manufacturing optician had solved that problem provided they could do the rest. The optician had said, "I calculate my lenses for 26 mm. in front; put it there and you will be satisfied." Whether that could be done in any particular case or not depended upon the patient. In many cases no doubt it could not be done, and he thought 26 mm. was too small an allowance.

**The Chairman** said his point was that there should be a different centre of rotation calculated for each condition of refraction. It was a non-significant difference to within fairly wide limits. They could group myopias under 5 D as being one centre, those over 5 D and under 15 D would have a second average centre, and those which were greatly elongated could be based upon a centre of rotation corresponding to what must be something like their average.

**Mr. Chalmers** thought that in the case of very high myopic eyes the eye would come up close to the lens, and something would be saved there. In the ordinary way, if the centre of rotation were pushed further back to the eye, the front came forward, and on the whole the lens would be able to be brought a little closer to the eye in the case of myopia—perhaps not as much as was necessary—but it would save a good deal of the difference.

**The Chairman** admitted that a case of myopia with prominent eyes could get his eyes nearer, but all the same he could not help feeling that the point had not been sufficiently considered and that no single approximation could be reasonably accurate for extreme defects of refraction, while it might be for errors of low degree.

**A Member** did not know that it was very relevant, but a great many people who had worn bi-concave or bi-convex lenses found that when they took to toric lenses they had a great deal of trouble. Many people could not wear toric lenses. Was that due to some inherent defect, or what?

**Mr. Chalmers** said if a man used ordinary lenses he did not mind the astigmatism much, but he minded it very much if, as often happened, this defect were over-corrected.

## "Luminous Dust."

**Mr. Green** said that Tscherning, in his paper, speaking of lenses of high curvature, pointed out that the only inconvenience of orthoscopic lenses arises from what we call false images, produced by rays which enter the eye after undergoing double reflection. They are of two kinds. In the first place, rays proceeding from exterior objects are reflected by the posterior surface of the lens, and afterwards return towards the eye by reflection at the anterior surface. These rays, as a rule, are not very troublesome. In the second place, the rays, which are reflected by the surfaces of the lens, arise from the eye itself and the neighbouring structures. In the case of orthoscopic lenses the surface of the eye, cilia, etc., are situated near the catoptric foci of the surfaces of the lens. Under good illumination some very pale images become visible, giving one the impression of a sort of luminous dust, which may be a little troublesome until one gets used to it. That might hold good for patients of the rotund and jovial type, but the nervous patient found such reflections extremely bothering, and some never got used to them.

**Mr. Dixey** said he appreciated Mr. Green's remarks, and no doubt he had some practical experience, but he never could himself see why the ghost-images should not be just as apparent in a bi-concave glass as in a periscopic or toric glass. But he would caution the meeting that that was a very lengthy subject to start at so late an hour.

**The Chairman** stated that Mr. Chalmers wished him to say that the points he had intended to raise in his own short paper which was down on the agenda—"Some Notes on the Accuracy required in Spectacle Lenses"—had practically been dealt with in his remarks in the discussion on Dr. Klughardt's paper. He therefore thought it was not desirable for him to read his paper and have a separate discussion upon it. They would all agree that they had had a very interesting evening, and a discussion on exactly those lines that the founders of the Society had in mind. They were all sincerely obliged to Dr. Klughardt for bringing this important matter before them in such a clear manner, and for affording them so interesting a basis for debate.